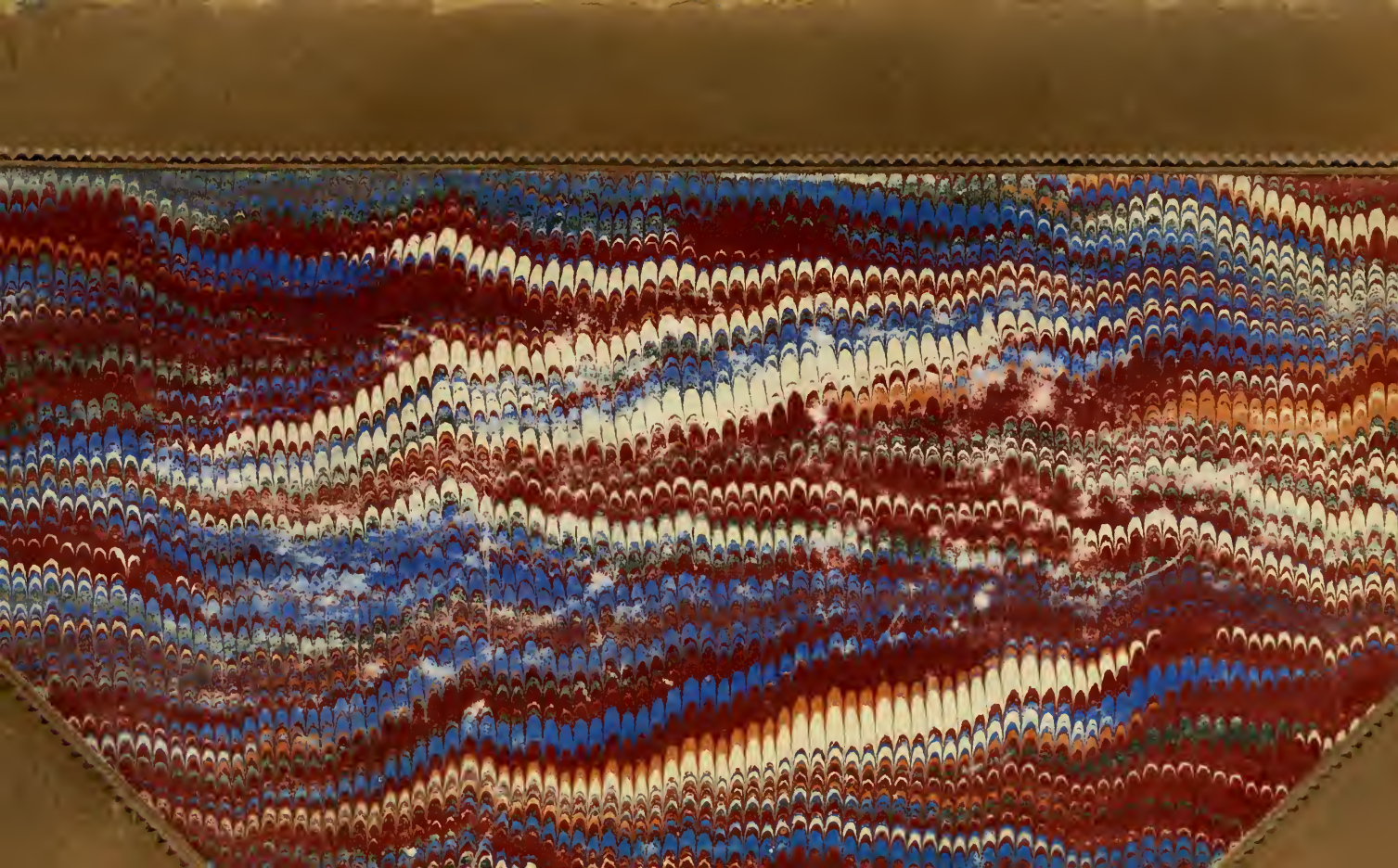

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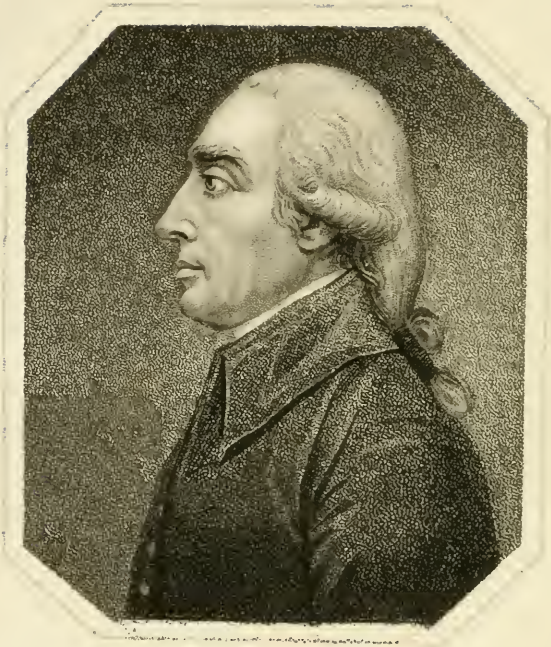

From Tassie. *Mackenzie sc*

Joseph Black . M.D.

Published Aug^t. 1801.

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, &c.

"Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. V.

LONDON:

PRINTED BY J. DAVIS, CHANCERY-LANE,

For ALEXANDER TILLOCH; and ſold by Meſſrs. RICHARDSON, Cornhill; CADELL and DAVIES, Strand; DEBRETT, Piccadilly; MURRAY and HIGHLEY, No. 32, Fleet-ſtreet; SYMONDS, Paternoſter-Row; BELL, No. 148, Oxford-ſtreet; VERNOR and HOOD, Poultry; HARDING, No. 36, St. James's-ſtreet; WESTLEY, No. 159, Strand; J. REMNANT, High-ſtreet, Bloomſbury; W. REMNANT, Hamburgh; and W. GILBERT, Dublin.

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THE

THE

PHILOSOPHICAL MAGAZINE.

OCTOBER 1799.

I. *Observations on Animal Electricity, and particularly that called Spontaneous.* By J. J. HEMMER *.

UNDER Animal Electricity I understand that kind observed in animals, from whatever cause it may arise. When this electricity is excited in animals, neither by the peculiar movement of their own bodies, nor by friction, or the application of any other body, it is called Spontaneous Animal Electricity. We are taught by many instances, both ancient and modern, that men, as well as other animals, have exhibited evident signs of electricity; although the ancients, who mention these instances, did not know to what the phenomenon was to be ascribed. It may not be improper, therefore, to quote here some of the most remarkable of them.

I. We are told by Virgil, that the hair of Ascanius emitted a harmless kind of flame †.

> Ecce levis summo de vertice visus Iuli
> Fundere lumen apex, tactuque innoxia molli
> Lambere flamma comas, et circum tempora pasci.
> Nos pavidi trepidare metu, crinemque flagrantem
> Excutere, et sanctos restinguere fontibus ignes.

* From the *Transactions of the Electoral Academy of Sciences at Manheim*, Vol. VI.

† Æneid, Lib. II. v. 582.

II. We are informed by Dionyſius and other writers, that fire ſtreamed forth from the hair of Servius Tullius, the Roman king, during ſleep, when he was about ſeven years of age *.

III. Pliny alſo ſpeaks of light often ſhining around the heads of men:—*Hominum quoque capita veſpertinis horis ſtellæ magno præſagio circumfulgent* †.

IV. We are told of a Carmelite monk, who was always attended with this phenomenon, that, as oft as he ſtroked his hair backwards, it emitted ſparks ‡.

V. A woman at Caumont exhibited a phenomenon of the like kind, as her hair, when combed in the dark, always emitted fire §.

VI. Father J. Faber ſpeaks of a young woman from whoſe head ſparks of fire always fell when ſhe combed her hair ‖.

VII. Franciſcus Guidus produced bright flames from his body when he rubbed his arm with his hand as he lay in bed ¶.

VIII. Ezekiel de Caſtro **, a phyſician of Verona, relates the following circumſtance reſpecting Caſſandra Buri, a lady of the ſame place:—As often as ſhe touched her body, even in a ſlight manner, with a linen cloth, it emitted ſparks in great abundance, which could be perceived by every perſon ſtanding near her, and which were attended with a conſiderable noiſe. Her maids were often deceived by this phenomenon, and believed that they had, through careleſſneſs, dropped ſome coals between the ſheets, as ſhe always cauſed her bed to be warmed in winter, at which time the ſparks were moſt abundant and ſtrongeſt.

IX. Anthony Cianſi, a bookſeller at Piſa, when he pulled off a narrow ſhirt, and a piece of cloth which he wore on his breaſt, emitted ſparks from his back and arms, with a crack-

* Antiq. Rom. Lib. IV.

† Hiſt. Nat. Lib. II. cap. xxxvii.

‡ Cardanus, Lib. VIII. de rerum variet. cap. xliii.

§ Scaliger. Exer. 174.

‖ In his Palladium Chymicum.

¶ Bartholinus de luce animalium, *Lugd. Bat.* 1647, p. 121.

** In his book *De igne lambente.*

ling

ling noiſe, to the great terror of his whole family. Fortunius Licetus was a witneſs of this phenomenon *.

X. "Among us," ſays Geſner, "where heated chambers are uſual, it often happens that many perſons, when they have warmed themſelves at a ſtove, and then pull off their ſhirt in a cold bed-chamber, or move or ſhake it after it is pulled off, obſerve crackling flames to burſt from it †."

XI. Bartholinus ſays, that a rope-dancer at Turin, according to the teſtimony of a reſpectable man, Caſſiano a Puteo, emitted a like phenomenon of light from his body ‡.

XII. The ſame author ſays, that ſparks proceed from the ſkins of cats, and particularly from the back; which can be clearly perceived by ſtroking the hair backwards even with the leaſt preſſure, and eſpecially after they have been warmed at the fire §.

XIII. Scaliger ſpeaks of a white Calabrian horſe, which, when combed in the dark, emitted ſparks of fire ‖.

XIV. Ezekiel de Caſtro ſays ¶, of another horſe of the ſame kind, that real ſparks were obſerved when his neck was ſtroked upwards with the hand or a currycomb.

XV. Simpſon treats of the light emitted by the bodies of animals when rubbed; and quotes inſtances of ſuch phenomena on combing the hair of a woman, currying a horſe, and ſtroking a cat with the hand **.

XVI. The following circumſtance reſpecting himſelf was told by Vaudania to the celebrated Beccaria:—"For ten or twelve days paſt, ſince the cold ſet in, I wear, between two ſhirts, a piece of beaver's ſkin. Always when I pull off my upper ſhirt at night, I obſerve that it adheres, in ſome degree, to the piece of ſkin; and when I draw my ſhirt from it, I ſee ſparks which have a ſtriking reſemblance to thoſe of electricity. Scarcely do I begin to pull off the piece of ſkin

* Licetus de cauſſis monſtrorum, Lib. II. cap. xxviii.

† Lib. de lunariis.

‡ De luce animali, p. 123.

§ Ibid. p. 189.

‖ Exercitat. 174.

¶ Liber de igne lamb.

** Diſſ. Phyſ. de fermentatione, 1675.

when I find that it adheres, and with ſtill greater force, to the under-ſhirt. On taking it out I obſerve, when I hold it in the right hand, that the frill of my ſhirt moves up from my body towards it. If I remove the piece of ſkin to a greater diſtance, and draw it from the frill, the latter moves again towards my body. If I bring the piece of ſkin nearer, the ſhirt moves again towards it. This oſcillation of my ſhirt between my body and the piece of ſkin continues alternately, till it is gradually leſſened, and at length ceaſes."

XVII. About twelve years ago the following circumſtance was communicated to our Academy, in writing, from Berdigheim:—On the 12th of February a young woman having put on an aired ſhift, which was exceedingly narrow and ſat very cloſe to her body, heard, on laying hold of it, a crackling noiſe like what oftimes proceeds from the flame of a candle, and obſerved ſparks to iſſue from all thoſe places which ſhe touched with her hands. Being aſtoniſhed at this phenomenon, ſhe called to another girl, who ſlept in the next apartment, to come to her aſſiſtance, as her ſhift, which ſhe in the mean time pulled off, was on fire. They both now ſhook the ſhift in order to extinguiſh the ſparks; but the more they moved it, the effects were ſtronger: on examining the ſhift, however, no traces of fire could be diſcovered. The firſt maid then put on another ſhift, which ſhe took from her box; but as ſoon as ſhe touched it, the ſame crackling noiſe and appearance of ſparks took place. She then put on her dirty ſmock, which ſhe had before pulled off, and returned to bed, on which the phenomenon diſappeared. Some perſons to whom the circumſtance was afterwards related, were deſirous to know whether the ſame phenomena would again appear if the maid put on another clean ſhift. She did ſo the following night; and as often as the ſmock was touched by her or any other perſon, a crackling noiſe was heard, and ſparks were emitted every time the finger was brought near it. The ſpectators now deſired her to put on another clean ſmock, which belonged to another maid: on approaching the hand a like noiſe was heard, and the appearance of fire was obſerved; but no ſparks iſſued from her body, even though touched, when ſhe had the ſhift on.

An

An experiment was then made to try whether the ſame phenomena would occur if another woman put on the girl's ſhift; but nothing of the kind was obſerved either by the perſon who had put it on, or by thoſe who touched it; but the phenomena were repeated when it touched the maid on whom they had been firſt obſerved. Every time after this period that the ſame perſon put on a clean ſmock, the ſame effects were produced; but they diſappeared after the ſhift had been worn ſome days. On the 1ſt of February the whole phenomenon ceaſed, and after that time never returned.

XVIII. M. Flad junior, member of our Society, having been in a conſumptive ſtate about eight years ago, obſerved, for a long time, as often as he pulled off or drew on his ſtockings, that ſparks proceeded from his feet in abundance. Having afterwards recovered his ſtrength in ſome meaſure, the ſparks diſappeared; but they returned when his ſtrength again began to decreaſe, and continued till his diſorder put an end to his exiſtence.

XIX. As often as M. Hertel, chaplain to the Elector, draws his hand over his breaſt, which is covered with hair, abundance of ſparks are ſeen in the dark.

XX. I was told by Count von Kagenek, that it often happened to him, that when he drew a ſilk handkerchief between his fingers, while ſtanding near a ſtove, long luminous ſtripes were here and there obſerved.

XXI. I was told alſo by a lady named Von Fraiſe, that very often, when ſhe rubbed her hands or arms even weakly with a linen cloth, lying in bed, abundance of ſparks iſſued from them.

XXII. M. Von Schlemmer, of Deux-Ponts, relates of a lady named Von Koch, that ſhe often obſerves, when ſhe gets up early, that a ribbon, with which ſhe binds up her hair during the night, adheres to her fingers when ſhe pulls it off, and moves with velocity towards other neighbouring bodies. Having, by my deſire, applied to the ribbon a piece of ſealing wax, which had been rubbed, it was always repelled by it.

XXIII. I am acquainted with another lady, whoſe hair, when

when combed in the dark, always has a luminous appearance, as I have ſometimes obſerved myſelf.

XXIV. I have a white horſe, from the body of which, particularly in the winter-time, the comb brings forth abundance of ſparks. I have likewiſe a white dog, from the back of which, when I draw my hand over it, in a warm room, from the tail to the head, ſparks iſſue with a ſnapping noiſe, and which ſeem ſo troubleſome to the animal, that he endeavours to run away. I have ſometimes charged a jar with them.

XXV. I have often produced the ſame phenomenon on an aſh-grey, ſtrong, lively cat.

XXVI. We are acquainted with three kinds of fiſh which, when touched, give a ſhock almoſt like that occaſioned by a charged jar, *viz.* the cramp-ray, *Raia torpedo* L.; the electrical eel, *Gymnotus electricus* L.; and the *Silurus electricus* L. The more modern philoſophers have convinced themſelves, by repeated experiments, that the effects of the ſhock given by theſe animals are of an electric nature, though to me it appears probable that they depend, in part, on another cauſe.

The animal electricity which I have announced in theſe numerous inſtances is of the coarſer kind, and may be eaſily obſerved by every one; but there is another kind, of a more delicate nature, which does not ſpontaneouſly manifeſt itſelf, but muſt be concentrated and called forth by art. This finer electricity I find firſt mentioned in a letter of M. de Sauſſure to the editor of the *Journal de Phyſique* at Paris. The experiments which this celebrated philoſopher made, partly on himſelf and partly on others, by means of Volta's electrometer and condenſer, are given there only in extracts; but the conſequences he deduces from them are as follows:

In order to produce this electricity, bodily motion is neceſſary. The electricity thence produced ariſes from the friction of the body againſt the clothes; for as often as he made the experiment naked, he found no electricity. In order that electricity may be produced by the friction of the body againſt the clothes, the latter muſt poſſeſs the natural warmth of the body; for when he had on clothes that were cold, he could never perceive the leaſt trace of electricity: no electricity

tricity appears alſo when the body is in a ſtate of perſpiration. There are perſons who, in this manner, never emit electricity. The electricity which ſhews itſelf in the human body is poſitive, and ſometimes negative. The cauſe of this variation he was not able to diſcover.

[To be concluded in next Number.]

II. *On the Method of Diſtilling as practiſed by the Natives at Chatra in Ramgur, and other Parts of India. By* ARCHIBALD KEIR, *Eſq.* *

THE body of the ſtill they uſe is a common, large, unglazed, earthen water-jar, nearly globular, of about twenty-five inches diameter at the wideſt part of it, and twenty-two inches deep to the neck, which neck riſes two inches more, and is eleven inches wide in the opening. Such, at leaſt, was the ſize of the one I meaſured; which they filled about a half with fomented Mâhwah flowers, that ſwam in the liquor to be diſtilled.

The jar they placed in a furnace, not the moſt artificial, though ſeemingly not ill adapted to give a great heat with but very little fuel. This they made by digging a round hole in the ground, about twenty inches wide, and full three feet deep; cutting an opening in the front, ſloping down to the bottom, on the ſides perpendicular, of about nine inches wide and fifteen long, reckoning from the circle where the jar was to come, to ſerve to throw in the wood at, and for a paſſage to the air. On the ſide too they cut another ſmall opening of about four inches by three; the jar, when placed, forming one ſide of it, to ſerve as a chimney for the ſmoke to go out at. The bottom of the earth was rounded up like a cup. Having then placed the jar in this, as far as it would go down, they covered it above, all round, with clay, except at the two openings, till within about a fifth of its height; when their furnace was completed.

In this way I reckon there was a full third of the ſurface

* From the *Aſiatic Reſearches.*

of

of the body of the ſtill, or jar, expoſed to the flame, when the fire came to be lighted; and its bottom, not reaching to within two feet of where the fuel was, left a capacious hollow between them, whence the wood, that was ſhort and dry, when lighted, being moſtly converted into flame, and circulating on ſo great a ſurface of the ſtill, gave a much ſtronger heat than could elſe have been produced from ſo very little fuel: a conſideration well worth the attention of a manufacturer, in our country more eſpecially, where firing is ſo dear. There indeed, and particularly as coal is uſed, it would be better, no doubt, to have a grate; and that the air ſhould enter from below. As to the benefit reſulting from the body of the ſtill being of earthen-ware, I am not quite ſo clear in it. Yet, as lighter ſubſtances are well known to tranſmit heat more gradually and ſlowly than the more ſolid, ſuch as metals, may not earthen veſſels, on this account, be leſs apt to burn their contents, ſo as to communicate an empyreumatic taſte and ſmell to the liquor that is diſtilled, ſo often, and ſo juſtly complained of with us? At any rate, in this country, where pots are made ſo cheap, I ſhould think them greatly preferable, as at leaſt much leſs expenſive than thoſe which the gentlemen engaged in this manufacture moſt commonly employ: though of this they are beſt able to judge.

Having thus made their furnace, and placed the body of the ſtill in it, as above deſcribed, they then luted on, with moiſtened clay, to its neck, at the opening, what they here call an *adkur*; forming with it, at once, a cover for the body of the ſtill, with a ſuitable perforation in it to let the vapour riſe through, and the under-part of the alembick. The *adkur* was made with two earthen pans, having round holes in their middles of about four inches diameter; and, their bottoms being turned oppoſite the one to the other, they were cemented together with clay; forming a neck of junction thus of about three inches, with the ſmall riſing on the upper pan. The lowermoſt of theſe was more ſhallow, and about eleven inches wide, ſo as to cover exactly the opening at the neck of the jar, to which they luted it on with clay. The upper and oppoſite of theſe was about four inches deep,

and fourteen inches wide, with a ledge round its perforation in the middle, rising, as is already said, from the inner side of the neck, of about half an inch high, by which a gutter was formed to collect the condensed spirit as it fell down; and from this there was a hole in the pan to let it run off by; to which hole they occasionally luted on a small hollow bamboo, of about two feet and a half in length, to convey it to the receiver below. The upper pan had also another hole in it, of about an inch square, at near a quarter of its circumference from the one below just spoken of, that served to let off the water employed in cooling; as shall be mentioned presently.

Their *adkur* being thus fitted to the jar, they completed the alembic by taking a copper pot, such as we use in our kitchens, of about five inches deep, eight wide at the mouth, and ten at the bottom, which was rather flattish; and turning its mouth downward, over the opening in the *adkur*, luted it down on the inside of the jar with clay.

For their cooler they raised a seat, close upon, and at the back part of the furnace, about a foot higher than the bottom of the copper pot. On this they placed a two or three gallon pot, with a round hole, of about half an inch, in the side of it; and to this hole, before they lighted their fire, they luted on a short tube of a like bore; placing the pot, and directing its spout, so as that, when filled with water, it threw a constant and uniform stream of it from about a foot high, or near the centre of the bottom of the copper pot, where it was diffused, pretty completely, over its whole surface; and the water falling down into the upper part of the pan of the *adkur*, it thence was conveyed, through the square hole already mentioned, by a trough luted on to it for that purpose, to a cooling receiver a few feet from the furnace; from which they took it up again, to supply the upper pot as occasion required.

As their stock of water, however, in this sort of circulation, was much smaller than it seemingly ought to have been, being scarcely more than six or eight gallons, it too soon became hot; yet, in spite of this disadvantage, that so easily might have been remedied, and the shortness of the conducting tube, which had nothing but the common air to

cool it, there ran a ſtream of liquor from the ſtill, and but very little vapour riſing from it, beyond any thing I had ever ſeen from ſtills of a much larger ſize, fitted with a worm and cooler. In about three hours time, indeed, from their lighting of the fire, they drew off full fifteen bottles of ſpirit; which is more by a great deal, I believe, than could have been done in our way from a ſtill of twice the dimenſions.

The conveniences of a worm and cooler, which are no ſmall expence either, I have myſelf often experienced; and if theſe could be avoided in ſo ſimple a way that might eaſily be improved, the hints that are here offered may be of ſome uſe. The thin metal head is certainly well adapted, I think, to tranſmit the heat to the water, which is conſtantly renewed; and which, if cold, as it ought to be, muſt abſorb the faſteſt poſſible: whereas, in our way, the water being confined in a tub, that, from the nature of its porous ſubſtance, in a great degree, rather retains than lets the heats paſs away, it ſoon accumulates in it, and becomes very hot; and, though renewed pretty often, never anſwers the purpoſe of cooling the vapour in the worm ſo expeditiouſly and effectually as is done by their more ſimple and leſs expenſive apparatus. In this country, more eſpecially, where labour and earthen-wares are ſo cheap, for as many rupees, and leſs, twenty furnaces, with ſtills, and every thing belonging to them, independent of the copper pots, might very well be erected, that would yield above a hundred gallons of ſpirits a-day; allowing each ſtill to be worked only twice. So very cheap indeed is arrack here, to the great comfort of my miners, and of many thoughtleſs people beſide, that for one ſingle *peyſa* (not two farthings ſterling) they can get a whole *cutcha-ſeer* of it in the *baſar*, or above a full Engliſh pint, and enough to make them completely intoxicated; objects often painful to be ſeen.

Of the ſuperior excellence of metal in giving out heat from itſelf, and from vapour contained in it, we have a very clear proof in what is daily performed on the cylinder of the ſteam-engine: for, cold water being thrown on it when loaded, the contained vapour is conſtantly condenſed; whence, on a vacuum being thus formed, and the weight of the atmoſphere

ſphere acting on the ſurface of the piſton attached to the arm of the balance, it is made to deſcend, and to raiſe the other arm that is fixed to the pump; while this, being ſomewhat heavier, immediately ſinks again, which carries up the piſton, while the cylinder is again filled; and thus, by alternately cooling and filling it, is the machine kept in motion; the power exerted in raiſing the pump-arm being always in proportion to the diameter of the cylinder, or to the ſurface of the piſton, which is exactly fitted to it, and on which the preſſure acts.

The contrivance, too, of having the under part of the alembic, where the condenſed vapour is collected, or upper part of what they call the *adkur*, of earthen-ware, of ſo great a thickneſs, and of courſe at ſo great a diſtance from the heat in the body of the ſtill, is well imagined to keep the ſpirits the cooleſt poſſible, when collected, and running off.

By thus cooling and condenſing the vapour, likewiſe, ſo ſuddenly as it riſes, there is, in a great meaſure, a conſtant vacuum made, or as much as poſſible can be; but, that both ſteam riſes faſter, and that water boils with much leſs heat when the preſſure is taken away from its ſurface, is an axiom in chemiſtry too well known to need any illuſtration; it boiling in vacuum when the heat is only ninety or ninety-five by Farenheit's thermometer; whereas in the open air, under the preſſure of the atmoſphere, it requires no leſs than that of two hundred and twelve, ere it can be brought to the boiling point.

I muſt further obſerve, that the ſuperior excellence of condenſing the vapour ſo effectually and ſpeedily in the alembic to our method of doing it in a worm and cooler, is greatly on the ſide of the former; both from the reaſons I have already adduced, and becauſe of the ſmall ſtream of vapour that can be only forced into the worm, where it is condenſed gradually as it deſcends; but, above all, from the nature of vapour itſelf, with reſpect to the heat contained in it, which of late has been proved, by the very ingenious Doctor Black, to be greater by far than, before his diſcoveries, was imagined. For vapour he has ſhewn to be in the ſtate of a new fluid, where water is diſſolved by heat; with the aſſiſtance,

perhaps, if I may be allowed a conjecture, of the air which it contains; and all fluids, as he has clearly demonſtrated, on their becoming ſuch, abſorb a certain quantity of heat, which becomes what he very properly calls latent heat; it being heat not appearing either to the ſenſes or to the thermometer while they remain in that liquid ſtate, but ſhowing itſelf immediately by its effects on whatever is near it; upon their changing their form from fluid to ſolid, as on water becoming ice, or metals fixing, and the like. In the ſolution of ſalts, alſo, there is an abſorption of heat, as we daily experience in the cooling of our liquors by diſſolving ſaltpetre in water; and this he has found to be the caſe with water itſelf, and other fluids, when paſſing into a ſtate of vapour by boiling. From the moſt accurate and judicious experiments, indeed, he infers, and with the greateſt appearance of truth, that the heat thus concealed in vapour raiſed by boiling, from any given bulk of water, would be fully ſufficient, if collected in a piece of iron of the like ſize, to make it perfectly red hot. What then muſt be the effect of ſo much heat communicated in our way of diſtilling to the worm, and to the water in the tub, will be ſufficiently evident, from what has been ſaid, to prove, I think, that we have hitherto employed a worſe and more defective method than we might have done with reſpect to cooling at leaſt, both in the making of ſpirits, and in other diſtillations of the like kind, where a ſimilar mode is adopted.

The poor ignorant Indian, indeed, while he with wonder ſurveys the vaſt apparatus of European diſtillers, in their immenſe large ſtills, worms, tubs, and expenſive furnaces, and finds that ſpirits thus made by them are more valued, and ſell much dearer than his own, may very naturally conclude, and will have his competitors join with him in opinion, that this muſt alone ſurely be owing to their better and more judicious manner of diſtilling with all thoſe ingenious and expenſive contrivances, which he can no ways emulate: but in this, it would appear, they are both equally miſtaken; imputing the effects, which need not be controverted, perhaps, to a cauſe from which they by no means proceed; the ſuperiority of their ſpirits not at all ariſing from the

the ſuperior excellence of theſe ſtills and furnaces, nor from their better mode of conducting the diſtillation in any reſpect; but chiefly rather from their greater ſkill and care in the right choice and proper management of the materials they employ in fermentation; and, above all, as I apprehend, from the vaſt convenience they have in caſks, by which, and from their abilities in point of ſtock, they are enabled, and do in fact, in general, keep their ſpirits for a certain time, whence they are mellowed, and improved ſurpriſingly both in taſte and ſalubrity.

All I need further add with reſpect to diſtillation, and on the ſuperior advantages in the mode of conducting it here, to that we have been in uſe to employ, for the raiſing of ſpirits, ſimple waters, and the like, is only to obſerve, I have no ſort of doubt but that the intelligent chemical operators at home, if ever they ſhould get a hint of it, will make no manner of ſcruple to uſe it alſo, and to improve upon it greatly by a few ingenious contrivances, which their knowledge and experience will ſo eaſily ſuggeſt. The principles on which it ſeems founded, indeed, eſpecially with regard to their way of cooling, are ſo ſtriking and juſt, that in many other diſtillations beſides thoſe of ſpirits and waters, they may be employed, I apprehend, with very great profit and advantage. I ſhall now, however, confine myſelf to mention only the benefit that may reſult from a like proceſs in the raiſing of the finer aromatics, while the heat contrived, as in our way, beſides impeding the diſtillation, muſt, from its long action on ſuch ſubtile bodies, probably injure them greatly in the eſſential quality on which their excellence depends; and upon this very account I am apt to imagine that the greater quantity obtained, and the ſuperior quality of the oil of roſes made in this country, to that made from roſes with us, is owing chiefly, if not entirely, to their better and more judicious manner of extracting it here. For with us, the ſtill, being made of metal, may, in the firſt inſtance, impart too great and too ſudden a degree of heat; and next, the oil continuing ſo long in the vapour, and that much compreſſed, may, in ſo delicate a ſubject, not only entirely almoſt unite it with the water, ſo as to render the ſeparation

tion impracticable, but may at the ſame time alter its eſſence ſo completely, as that it can no longer appear in the ſtate it otherwiſe might have been found in, had the operation been better conducted, or in the way they do here. A very few trials, however, would much better certify this than all I can poſſibly ſay on the ſubject, or, in fact, than all the reaſoning in the world. Therefore, as to my own particular opinion of the flavour and quality of the roſes at home being equal, if not ſuperior, to that of thoſe in this country, I may be entirely ſilent; the rules and reaſoning in chemiſtry, though ſerving greatly to enlarge and improve our underſtanding, being what of themſelves can never be depended upon till confirmed by facts and experiments; where many things often turn out very different from what, from our beſt and moſt plauſible arguments, we had the greateſt reaſon to expect. Or, if it ſhould be found to be really true, what I have often heard aſſerted, by thoſe however who had it only from others, but not of their own particular knowledge, that, in diſtilling their oil of roſes at the places where they make it the beſt, they uſe alſo with their roſes, ſandal wood, and ſome other aromatics, no roſes whatſoever, it is plain, could ever of themſelves be made to afford a like oil, nor without ſuch an addition as they employ. A circumſtance, by the bye, that might poſſibly eaſily be certified by ſome one of the many ingenious correſpondents of the Society who may happen to reſide where it is made; and a knowledge of the real truth of it would certainly be of uſe.

III. *On the Method employed at Fez and Tetuan to prepare thoſe Goat Skins called Morocco.* *By* C. A. Brousonet *.

THE ſkins, after being flayed from the animals by ſtripping them off entire, are immerſed in water for three days: they are then expoſed to the air, and, when dry, the hair is taken off, but imperfectly. They are then dipped in ſlacked lime, and ſprinkled over with powdered lime to detach the

* From the *Bulletin des Sciences*, No. 23.

ſmaller hairs; after which they are waſhed in running water, and rinſed with care. After remaining a night in the water, they are dried in the open air. Thirty parts of theſe ſkins are then placed in two quintals of bran, (each part conſiſts of ſix ſkins, and the quintal is 150 pounds.) They are ſuffered to continue in this ſtate, turning them every day, till they have acquired a great degree of pliability. They are afterwards waſhed again in running water, and trod upon with the feet; then thrown into a ſecond bath made with white figs, about a quintal and a quarter of which are employed for thirty parts of ſkins. The figs render the water ſaponaceous. They are left in this bath four or five days, during which they are often turned; and, while immerſed in the water, they are for three days beſprinkled with finely pounded rock-ſalt. The water is then drained off, and, being again beſprinkled with ſalt, they are placed in a heap in a flat veſſel, where they imbibe the ſalt completely. The water they contain is wrung out by twiſting them; and they then become exceedingly pliable, and fit for receiving the colour.

If deſtined to be red, half a pound of cochineal and three ounces of alum are employed for ten parts of ſkins. They are then put into pits, where tan-beds are formed of about fifty pounds for each ſkin; which is turned in ſuch a manner that the grain is inſide, and that the ſkin may be filled with the tanning water. At the end of eight days they are turned inſide out, and are ſtill filled with the tanning water, in which they are left for ſix days; care being taken to ſtir them. They are then rinſed in running water; ſcraped with an iron inſtrument; cut up along the belly; and ſoftened with a little oil. They are dried in the ſun, and then cooled in the ſhade: after which they are gently beſprinkled with water, and the edges are pared off with three different inſtruments of iron. When the red is too dark, a decoction of a plant called *razoul al achbi*, a kind of annual *meſembrianthemum*, is employed to weaken it. This liquid is applied warm, by pouring a ſpoonful over each ſkin.

If the morocco is intended to be yellow, the ſkins are prepared as for the red, but not ſalted till they are put into the fig-bath; and for five dozen of ſkins, twenty-five pounds of

tan

tan only are employed. They are dyed with the pulveriſed bark of the pomegranate tree and alum.

The falſe red dye is communicated to ſkins with Brazil wood and alum. Inſtead of Brazil wood, *fouah*, a kind of *galium* or *rubia* (madder), brought in large quantities from Morocco, is often employed.

IV. *Recipe for deſtroying Caterpillars on Gooſeberry Buſhes*.*

A RECEIPT for this purpoſe was offered to be communicated to the Society by William Henderſon, of Baldridge Burn near Dunfermline, on the 6th of February 1795, for a ſuitable reward. The propoſal was referred to a Sub-Committee, of which Dr. Monro, Profeſſor of Anatomy in the Univerſity of Edinburgh, was chairman, who, after making trial of the receipt, gave in their report on the 1ſt of July 1796. The receipt for the preparation, and the manner of uſing it, was in the following words:—

Take one Scots pint of tobacco liquor†, which the manufacturers of tobacco generally ſell for deſtroying bugs, and mix therewith about one ounce of alum; and when the alum is ſufficiently diſſolved, put this mixture into a plate, or other veſſel, wide and long enough to admit of a bruſh, like a weaver's bruſh, being dipped into it; and as early in the ſeaſon as you can perceive the leaves of the buſhes to be in the leaſt eaten, or the eggs upon the leaves, (which generally happens about the end of May,) and which will be found in great numbers on the veins of the leaves on their under ſide; you are then to take the preparation, or liquor, and after dipping the bruſh into it, and holding the bruſh

* From *Prize Eſſays and Tranſactions of the Highland Society of Scotland*, Vol. I.

† Tobacco liquor is the ſuperfluous moiſture expreſſed from roll tobacco in the operation of preſſing it; and is, in fact, only a ſtrong infuſion of tobacco in well or ſpring water, which may be made, where it cannot be purchaſed, by infuſing any kind of tobacco in water till all the ſtrength be extracted. Perhaps the ſulphat of iron (copperas), employed in dyeing the roll tobacco, contributes a little to the efficacy of the liquor: a little of it may therefore be added to the infuſion. EDIT.

towards

towards the under ſide of the buſh, which is to be raiſed and ſupported by the hands of another perſon; and by drawing your hand gently over the hairs of the bruſh, the above liquid is ſprinkled and falls in ſmall drops on the leaves: the conſequence of which is, if the eggs are there, they never come forward; and if they have already generated worms, in a minute or two after the liquor touches them, they either die, or ſicken ſo as to fall off the buſh, at leaſt they do ſo upon giving it a little ſhake. If, upon their thus falling off, they ſhall not appear to be completely dead, the buſh ſhould be held up, and either a little boiling water from a watering-pan thrown on them, or a bruiſe given them by a ſpade or ſhovel, or the earth where they lie turned over with a hoe. This preparation does not in the leaſt injure the buſhes.

The liquor here meant is generally not in the ſame ſtate it is extracted from the tobacco, but is mixed by the tobacco manufacturers with cold water, in the proportion of four or five pints of water to one of the original juice or eſſence. Therefore, any perſon who may purchaſe the juice itſelf, unmixed, muſt mix it with water in the above proportion, and the quantity of alum muſt be about an ounce for each Scots pint of the mixture.

Dr. Monro's report was in the following words:—I obſerved, along with Mr. Hamilton and Mr. Gordon, (two other gentlemen of the committee,) and two gardeners who were preſent, that ſuch caterpillars as were wetted by the liquor Mr. Henderſon employs, were killed in a very few minutes, and the experiment has been repeated by my own gardener with the ſame effect. I have likewiſe found, that it kills a kind of green fly, which is very hurtful to the leaves of plum-trees and other fruit-trees. It has been very generally known, that the ſmoke and the juice of tobacco were pernicious to different kinds of inſects and worms; but it has not, ſo far as I know, been employed in Mr. Henderſon's manner; and as this has the advantage of not hurting the leaves, nor the fruit, I conſider it as an uſeful and material improvement, well entitled to a moderate premium. (Signed) ALEX. MONRO, M.D.

V. *On the gradual Changes in Temperature and Soil which take place in different Climates, with an Enquiry into the Cauſe of thoſe Changes. By the Abbé* MANN.

[Concluded from Page 347 of the laſt Volume.]

II. *Phyſical Cauſes of the gradual Alterations in Soil and the Temperature of the Climate.*

I HAVE no doubt that a great number of different cauſes may have contributed, each its part, more or leſs, to produce thoſe effects which are the object of the preſent research. Some of them are only accidental, and have taken place in different countries at very different periods, while in others ſome of them have not taken place at all. Among theſe are the draining of lakes and moraſſes, the extirpating of foreſts, and the cultivation of land. All theſe circumſtances, no doubt, render the temperature of climate milder. I found, however, one cauſe, of a kind altogether different, which appears to me general and uniform in producing the above changes. After what I have before ſaid, and in another place*, it may be readily conjectured that I here ſpeak of an union of the two diſtinct principles, moiſture and heat. Their mutual diſengagement, and the increaſe of the one above the other, afford, if I am not miſtaken, a key to a true theory of the earth.

All the ancient writers who ſpeak of the countries of Europe beyond the latitude of 50° north, repreſent them as filled with lakes and moraſſes, and covered with immenſe foreſts, almoſt as America is at preſent. It is a certain fact, that the climate of North America is different from that of Europe by about ten degrees of longitude; that is, the diſtricts of North America lying under 40° of north latitude are as cold and moiſt as the countries of Europe which lie in the latitude of 50°. New England lies between the 41ſt and 46th degrees of latitude; yet it is obſerved that the climate there, in regard to heat and cold, is equal to that of

* See my Memoir on the Ancient State of Flanders in *Memoires de l'Acad. de Bruxelles*, Vol. I. p. 67—72.

the

the diſtricts of England between the latitude of 50 and 56. It is well known that the people of America are more and more extirpating the foreſts, draining the marſhes, and cultivating the land; and that the climate there is found to become perceptibly milder. For a thouſand or two thouſand years paſt the people in all the northern parts of Europe have been in the ſame manner employed in the improvement of the ſoil. Theſe cauſes, however accidental they may be, and however much dependant on human induſtry, muſt certainly have contributed their part to render the climate milder, not only in the countries where they took place, but even in the neighbouring countries, expoſed to the effects of their atmoſphere.

It is almoſt needleſs to obſerve, that the great number of lakes and moraſſes, which, according to the accounts of ancient authors, exiſted in their time in the ſouthern parts of Europe, muſt have rendered the air of theſe countries exceedingly cold and moiſt as well as unhealthful, ſince it leſſened its elaſticity, and filled it with thick vapours; which correſponds with the deſcription they have given us of them. The countries of Europe have not for a long time been ſeen covered in that manner with lakes and moraſſes, if we except Sweden and Norway; though the places where ſuch marſhes formerly exiſted, both in England and on the Continent, in Gaul, Germany, and the European Sarmatia, may be ſtill clearly obſerved. It is certain that human induſtry, in the courſe of ages, has in part contributed to produce this change, as a great many epochs are known when the draining of lakes and moraſſes was undertaken; but I have no doubt that gradual ſinking of the ſurface of the ſea, which occaſions a natural and gradual efflux, that could be produced only by theſe means, may have contributed its part alſo. But, from whatever cauſe theſe changes may have proceeded, it is certain that they have contributed to leſſen the moiſture and cold of all the countries of Europe.

We know that in the time of Julius Cæſar, and even long after, almoſt all Germany and Sarmatia were covered with immenſe foreſts. The Hercynean foreſt was ſixty days journey in length. It began in Belgic Gaul near the ſea,

and extended through Germany and Poland. England was proportionally leſs abundant in foreſts. Now it may be readily comprehended what extraordinary cold, what moiſt and unhealthful air muſt have prevailed in the climate of theſe extenſive countries, as all the mountains and plains were covered with ſuch immenſe woods, and as each valley almoſt contained a lake or a marſh; and what wonderful changes in the temperature of theſe lands muſt have been effected by the extirpation of theſe extenſive foreſts, and by draining off the ſtagnant waters. Large woods prevent the beams of the ſun from penetrating to, and warming the ſoil; they impede alſo the free diffuſion of the internal heat, as the fallen leaves and branches which rot on the ground form a moiſt craſt through which the internal and external heat can with difficulty force a paſſage. In the laſt place, they concentrate the cold and moiſt vapours, render them putrid, and corrupt the whole atmoſphere. This has been always obſerved in North America, as we are aſſured by Dr. Williamſon; and the conſequences are bilious and intermittent fevers in ſummer and autumn, and inflammatory fevers in winter. He aſſerts, that the opener and drier the land becomes, the more it is remarked that theſe fatal diſeaſes decreaſe. This muſt have been the caſe formerly in Europe under the like circumſtances, and the like cauſes muſt have contributed to render its climate milder and more ſalubrious.

The Celts and Sarmatians, who were the firſt inhabitants of all the European countries lying to the north of Italy and Greece, like all the barbarous nations under different names which deſcended from them, and which over-ran the Roman empire in the fifth and ſixth centuries, deſpiſed agriculture, and cultivated no more land than was ſufficient to ſupply the wants of the current year. They lived chiefly either on what they caught in hunting, or the fleſh of their domeſtic animals, of which they reared a great many: as they conſidered, though very unjuſtly, theſe employments as much nobler than the cultivation of land. Now it is certain that the culture of the earth, which breaks its ſurface, puts it in movement, keeps it in a ſtate of continual tenderneſs, and makes it capable of imbibing the rays of the ſun in ſummer, and of affording

affording a paſſage to the internal heat in winter; and by theſe means contributes to preſerve a continual equilibrium of the principle of heat in the earth and the atmoſphere. The contrary takes place in all uncultivated countries, eſpecially when they are moiſt, and covered with wood.

It can no longer be doubted that the gradual draining of the ſtagnant water in all Celto-Scythia and European Sarmatia, with the extirpation of their large foreſts, and the general cultivation of the fields of theſe countries, muſt have had an influence alſo on the atmoſphere of Greece and Italy. Thoſe cutting north-winds which converted every thing into ice, and of which the Greeks and the Romans complain ſo much, have, in a great meaſure, ceaſed ſince the principal cauſes which produced them no longer exiſt. As long as Germany, Pannonia, Dacia, Mœſia and Thrace remained uncultivated and covered with immenſe foreſts, their atmoſphere was exceedingly cold, thick and heavy, and had a continual influence on that of Italy and Greece, in which, becauſe they were open and warm countries, the atmoſphere was conſequently far lighter. The exertions of this fluid to recover its equilibrium were the cauſe of the cutting north winds, of which the Greeks and the Romans complained ſo much. But after thoſe centuries, when the whole of Celto-Scythia and Sarmatia became opener and better cultivated, their atmoſpheres muſt have come nearer to an equilibrium with that of Greece and Italy, and conſequently theſe ſtreams of air from the north muſt have decreaſed in the ſame proportion. This muſt have contributed to moderate the climate of Greece and of Italy, and to render it much milder than it was about 1800 or 2000 years ago; and to ſuch a degree, that, had there been no other cauſe, we could no longer wonder at, or entertain any doubt of, the effects of the cold which the ancients remarked in their time, and which are not obſerved at preſent.

It is well known that the winds which traverſe the immenſe cold regions of the Continent are always colder and more penetrating than thoſe which blow from the ſea. America extends a great way towards the north; and its remote diſtricts, on account of the ice, ſnow, and continual

fogs

fogs which prevail there, have never been explored, and therefore remain unknown. This immenſe country certainly extends nearer the north pole than the Continent of Europe and Aſia, the boundaries of which are pretty well known. This, without doubt, may be reckoned among the cauſes which render North America much colder than Europe under the ſame degrees of latitude, as I have already remarked. Hence it happens that the immenſe Continent of America, the farther it extends towards the north, is more filled with lakes and marſhes, which muſt conſequently add to the ſeverity of the winds that ſweep their ſurface.

It will perhaps be ſaid, that it appears to follow, from my poſitions, that the more the cold of winter decreaſes, from the above cauſes, the heat of ſummer ought to increaſe in the ſame proportion. In my opinion it can be proved by many monuments, hiſtorical as well as phyſical, that the ſum-total of the mean ſummer heat is greater than it was formerly, and that it continually increaſes; though this takes place imperceptibly, and can be obſerved only after periods of conſiderable length, and by comparing the reſpective degrees of diſtant epochs. On the other hand, in regard to the degree of the intenſity of that penetrating, concentrated, and ſtifling heat which is experienced even in Lapland, I muſt ſay, that this kind of heat is leſſened by all thoſe cauſes before enumerated which leſſen the cold of winter. I by no means aſcribe contrary effects to the ſame cauſe. We are taught by general experience, that the thinner, purer, and more elaſtic the air is, the leſs, in the ſame proportion, is the intenſity of the ſummer heat; and, on the contrary, the thicker the atmoſphere is, and the more it is filled with ſtagnant and concentrated vapours, the heat is more intenſe and the more ſtifling. For this reaſon it is always cooler on the ſummits of high mountains, while a ſtifling and inſupportable heat prevails in the neighbouring plains, eſpecially when they are ſurrounded by wood. This is always obſerved in the ſavannahs of America. Dr. Williamſon was convinced, by experiments and obſervation, of the truth of my aſſertions in regard to North America: and he remarks, that when this extenſive country becomes entirely open, when its

woods are cut down and its plains cultivated, the severity of the winter cold will not only decrease, but the stifling unhealthful heat of the summer will be moderated. The quantity of the snow, ice, and moisture is already evidently lessened; and many plants, which could not be cultivated there formerly, now thrive and succeed.

I now come to the last and principal cause of these changes, which acts in a general and uniform manner in producing these effects, as all the others are merely accidental, and depend on human industry. I here mean to say, that the principle of heat, increased continually in the course of time, so as to overcome the opposite principles of moisture and cold, renders, by these means, the earth drier and fuller of stones, and consequently increases the sum of the degree of heat. Without this principle, in my opinion, we can never find sufficient grounds for the wonderful changes which have taken place in the nature of the soil of all those lands which border on the Mediterranean sea, which formed the ancient empire of Rome from Syria to India, and which at present have all become uncommonly fruitful, dry and stony, as I have already remarked. The mere neglect of agriculture could never have produced these effects, and must have been attended rather with effects of a contrary nature *.

VI. *Agenda,*

* The author here subjoins the different passages of ancient authors alluded to in the course of this paper; but as these would occupy too much room, we must content ourselves with giving only the references, which will no doubt be acceptable to our learned readers, who may be desirous of prosecuting this subject farther:—Herodotus (469 years before the birth of Christ), Lib. IV. cap. 28, 29; M. Terentius Varro (72 years before J. C.), De re rustica, Lib. I. cap. 7; C. Jul. Cæsar (52 years before J. C.), De bello Gallico, Lib. IV. cap. 1; Virgil (30 years before J. C.), Georgic. Lib. III. v. 349—383; Georgic. IV. v. 122, 135; Diodorus Siculus, (45 years before J. C.), Biblioth. Hist. Lib. V. cap. 25; Ovid (10 years before J. C.), Trist. Lib. III. Eleg. IV. v. 48, 49. 51; Eleg. X; Strabo (28 years after J. C.), Geograph. edit. *Basil*, 1539, Lib. II. p. 67, 68. 107 119, Lib. VII. p. 297; Pomponius Mela (40 years after J. C.), De situ orbis, Lib. II. cap 1. De Scythia Europæa; cap. 2. De Thracia; Lib. III. cap III. De Germania; Columella (43 years after J. C.), De re rustica, edit. Stephani 1543, in præf. n. 7, 8. Lib. I. cap. 1. p. 11, 12; L. Ann. Seneca (55 years after J. C.), De providentia, cap. 4. edit. *Lugd. Bat.* Tom. I. p. 711; Petronius Arbiter (60 years after J. C.), Satyr.

VI. *Agenda, or a Collection of Observations and Researches, the Results of which may serve as the Foundation for a Theory of the Earth.* By M. De Saussure.

[Continued from page 359 of the last Volume.]

CHAP. XXI.

Researches to be made in regard to the Loadstone.

1. THE theory of the loadstone ought to form a part of the theory of the earth, because the phenomena which depend on it belong to the whole globe; and because Halley, and after him other philosophers, have endeavoured to explain the different phenomena of the magnet by supposing the earth to be hollow, and that it contains in its cavity one or more magnetic globes.

2. In considering the loadstone it ought first to be examined whether, in order to explain its phenomena, we ought, like Descartes, to suppose a close fluid moving in a vortex around the magnet, and entering at one of its poles and issuing at the other; or, as M. Æpinus, a discrete fluid, susceptible of rarefaction and condensation, which is rarefied in one of the poles and condensed in the other; or, lastly,

Satyr. p. 10; Pliny the elder (74 years after J. C.), Hist. Nat. edit. *Basil* 1525, fol. Lib. IV. cap. 12. p. 60; Papinius Statius (85 years after J. C.), Sylv. Lib. V. p. 83. edit. *Amst.* 1624; Tacitus (97 years after J. C.), De moribus Germanorum, cap. 2. 4. 5; Pliny the younger (99 years after J. C.), in Panegyr. cap. 12; Plutarch (101 years after J. C.), De fluviis, Tom. II. p. 1156. edit. *Frankf.* 1620, p. 949; L. A. Florus (102 years after J. C.), Lib. IV. cap. 12. edit. *Elzev.* 1610, p. 440; Appianus (130 years after J. C.), Excerpta ex ejus Celticis a Valesio, p. 1220; Pausanias (174 years after J. C.), Arcad. cap. xvii. p. 634. edit. *Lipsi.* 1696, fol.; Dio Cassius (238 years after J. C.), Hist. Lib. XLIX. p. 413. edit. *Hanov.* 1606. fol.; Herodian (229 years after J. C.), Hist. edit. *Oxon.* Gr. & Lat. 1699. 3. Lib. I. p. 12. Lib. VI. p. 221; Justin (250 years after J. C.), Histor. T. II. cap. ii. p. 25. edit. *Elz.* 1664; Ammianus Marcellinus (370 years after J. C.), Lib. XIX. cap. 11; Jornandes (525 years after J. C.), De rebus Geticis, cap. liv. p. 693. edit. *Amst.* 1655; Xiphilinus (1063 years after J. C.), in Epit. Dionis Cassii, Lib LXVIII. p. 776, and Lib. LXXI. p. 804.

as M. Prevoſt*, two fluids, ſuſceptible of being combined with each other in ſuch a manner that one of them alone is accumulated around the north pole of a magnet, while the other is accumulated around the ſouth pole; and that all the magnetic phenomena may be explained by the elective attractions which theſe fluids exerciſe either upon each other or on iron †.

3. It muſt then be examined, whether the direction of the magnetic needle, and its inclination, depend on the ſituation of a large magnet encloſed in the bowels of the earth, as Halley ſuppoſes; or on the action of one of theſe two magnetic fluids towards one of the poles, and, perhaps, of the other fluid towards the oppoſite pole, as M. Prevoſt ſuppoſes.

4. If we admit the hypotheſis of a large magnet ſuſpended within the cavity of the earth, ſhall we ſuppoſe, as the inventor of this hypotheſis, that this magnet has four poles? Or, ſhall we endeavour to explain the whole, as that great geometer Euler has done, by a magnet having only two poles? Or, laſtly, ſhall we ſuppoſe, as Mr. Churchman, an American philoſopher, has lately done, that the earth contains two magnetic poles, one at the north and the other at

* De l'origine des forces magnétiques, 8. *Geneve* 1788.

† The celebrated Coulomb admits alſo two fluids, which compoſe the magnetic fluid, and which exerciſe their action in the inverſe ratio of the ſquare of the diſtance: but, in the theory of phenomena connected with the action of the globe, this philoſopher ſets out from certain facts immediately given by obſervation; one of which it would be of importance to verify in different points of the globe. This fact is, that the forces which attract one of the poles of a magnetic needle freely ſuſpended towards the north, are equal to thoſe which attract the oppoſite pole towards the ſouth. Coulomb concludes that this equality exiſts, becauſe a needle, weighed two different times, before and after it was magnetiſed, was found exactly of the ſame weight.

C. Borda has found, by obſervations made firſt at Breſt, Cadiz, Teneriffe, Goree on the coaſt of Africa, and afterwards at Breſt and Guadaloupe, that the intenſity of the force exerciſed by the globe on the magnetic needle, eſtimated according to the number of oſcillations made by the needle in a given time, was ſenſibly the ſame in theſe different places. This obſervation in other latitudes, eſpecially on approaching the poles, might throw ſome light on the theory of natural magnetiſm.—Note of C. Hauy.

the ſouth, at different diſtances from the poles of the earth, which perform their revolutions in different times; and that, from the combined influence of theſe two poles, we may conclude the annual changes of the declination with ſo much preciſion, that we can deduce the longitude of any place from its latitude, and from the degree of declination which the needle experiences *.

5. Thus, by ſuppoſing one or more magnets in the interior part of the earth, the annual changes of the declination and inclination may be explained by the rotary movements of theſe magnets. But in the ſyſtem, which does not admit theſe internal magnets, it is aſked, Whether the changes of declination might not depend on movements which produce the change of obliquity, preceſſion, nutation, and perhaps other phenomena or inequalities of that kind †.

6. With regard to diurnal variations, an Engliſh philoſopher, Mr. Canton, conſidering that it is proved by experience that heat diminiſhes the force of the magnet, thought that the ſolar rays, by heating the earth, muſt leſſen the attractive force of the grand magnet contained in it; and he thence deduced, as will be ſeen hereafter, an explanation of theſe variations. But Mr. Canton did not reflect on what

* Heads of Lectures by S. Prieſtley, *London* 1794.

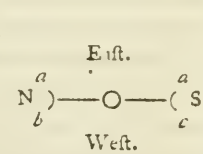
† Æpinus gives another explanation independent of theſe movements. It may be poſſible, according to this philoſopher, that the declination of the magnetic needle ariſes, in general, from the irregular figure of the nucleus of the magnetic globe, or from an unequal diſtribution of the fluid in its interior part: and to account for the variation of this declination in one place, in the courſe of time we might ſuppoſe that the figure of the nucleus, or the diſtribution of the fluid it contains, is itſelf variable. Æpinus preſumes alſo, that the action of the iron-mines diſperſed throughout the boſom of the globe, may have an influence on the variation in queſtion; and may, perhaps, be the ſole cauſe of it. *Tentamen theoriæ electr. et magnet.* p. 268, 271, 334.

This philoſopher wiſhes that men of ſcience, who have an opportunity of being near a mine of loadſtone, would determine, by obſervation, whether the maſſes of this mineral, before they are taken from the bowels of the earth, have their poles diſpoſed, in regard to the poles of the world, like thoſe of needles freely ſuſpended: and whether, in certain maſſes, the poles are not in an inverſe direction, of which he ſhews the poſſibility by means of conſequent points. *Ibid.* p. 333.—Note of the ſame.

was

was clearly ſeen by M. Æpinus, that this magnet, if it exiſts, is ſunk to too great a depth in the earth for the action of the ſolar rays, or at leaſt the variations of that action, in the morning and evening, to be able to penetrate to it. We may, however, apply to the ferruginous minerals, diſperſed in abundance over the ſurface of the earth, what Mr. Canton ſuppoſed, in regard to the grand magnet contained in its boſom; and then, if we admit that theſe minerals exerciſe any action on the magnetic needle, we cannot deny that the heat, excited by the rays of the ſun, may diminiſh that action. From theſe principles it would follow, that in the morning, when the ſun warms the ſurface of the ground ſituated to the eaſt of the needle, the latter, being leſs ſtrongly attracted towards that part, ought to decline towards the weſt; and, for a contrary reaſon, it muſt in the evening decline towards the eaſt. But Mr. Canton proved, by a long ſeries of obſervations, that at London, at leaſt, this is the ordinary courſe of the diurnal variations.

7. But it will be proper to examine whether this explanation, even thus corrected, does not contain a paralogiſm; and when the attractive force of all the ferruginous particles, diſperſed over the ſurface of the earth, to the eaſt of the needle, is diminiſhed equally and ſimultaneouſly, the needle ought not to remain motionleſs; ſince the diminution of the attraction exerciſed upon the ſouth-pole of the needle, compenſates for the diminution of that exerciſed upon the north-pole*.



* Let ○ be the centre of ſuſpenſion of the needle N.S., and *a*, *b*, *c*, *d*, the forces which attract the needle in oppoſite directions; for example, pieces of iron. The forces in *b* and *d* conſpire to make the extremity N of the needle move towards the weſt; and the forces at *a* and *c* conſpire in the like manner to make the ſame extremity proceed towards the eaſt. When the needle remains at reſt, there is an equilibrium, and the forces $a + c = b + d$. But in this ſuppoſition, if the forces of the ſame ſide, *b* and *c* for example, gradually diminiſh, the equilibrium will not be interrupted. For, let $b = y + m$ and $c = z + m$: if the forces *b* and *c* are equally diminiſhed by the quantity *m*, we ſhall always have $a + z = b + y$. The caſe will be the ſame with any augmentation, if it be equal and ſimultaneous on all the ſides of the needle. —Note of the Author.

I ſay the ſame thing of thoſe ſituated to the weſt. If this reaſoning be juſt, the needle ought not to vary by the action of the ſolar heat, but when this heat diminiſhes the magnetic force of the ferruginous parts ſituated to the north of the needle, more than that of the parts ſituated to the ſouth, or reciprocally.

To determine this curious queſtion, it would be neceſſary to make choice of two oppoſite coaſts, and directed almoſt eaſt and weſt from the magnetic meridian; ſuch as the coaſt of Provence to the ſouth, and that of Normandy to the north; to eſtabliſh two compaſſes well ſuſpended, ſuch as thoſe of M. Coulomb, one to the ſouth, at Antibes for example, and the other to the north, near Cape de La Hogue; and to ſee whether their diurnal variations did not proceed in a contrary direction: that is to ſay, whether that at Antibes, having the continent to the north, and only ſea to the ſouth, would not decline, in the morning, towards the weſt, as that of Mr. Canton did; and whether that of La Hogue, having the continent to the ſouth and ſea to the north, would not, at the ſame time, decline to the eaſt. Mr. Canton, indeed, who made theſe obſervations at London, had, to the north of his magnetic horizon, the greater part of England and all Ireland; and thus he muſt have had the variation weſt in the morning and eaſt in the evening, as he obſerved it; for it is certain that the ſea preſerves the land, which it covers, from the action of the ſun; and that thus the attraction of that land ought not to vary by the heat which emanates from that luminary.

By carefully repeating and varying theſe obſervations, in places choſen with diſcernment, we ſhall be able to decide whether the regular diurnal variation depends upon a general cauſe, the action of which, however, is ſuſceptible of being ſuſpended or diſturbed by local cauſes; or whether we are to believe, on the contrary, as M. Van Swinden does, that the diurnal variation is not a coſmic phenomenon, or that it does not depend on a general cauſe inherent in the globe, and which every where acts according to the ſame law.

9. Is there properly any action of the magnetic fluid on the electric fluid? or is there, between theſe two fluids, only a reſemblance of properties, or in their manner of action?

10. Is it well aſcertained, as M. Van Swinden thinks, that the *aurora borealis* acts on the magnetic needle; and can any idea be formed of the mode of this action?

11. The ſame queſtion in regard to the zodiacal light.

12. In general, the theory of the magnet is ſtill ſo far from perfection, even in that part which depends merely on obſervation, that it is much to be wiſhed that obſervers and obſervations were multiplied, eſpecially in what concerns the inclination of the needle. In regard to the declination and its variations, M. Van Swinden has given a noble example of correctneſs, and of conſtancy in obſervations, and in the art of claſſing and comparing the reſults. It would be of great benefit if this example were followed in different ſituations and climates. It would be highly intereſting, for inſtance, to determine, with preciſion, the zones of the earth where the declination is nothing, and where changes take place, and the ſame for the inclination.

[To be continued.]

VII. *On the Effects produced on different Kinds of Stones by the Vapour of the Fluor Acid. By* M. KORTUM *of Warſaw**.

HAVING employed myſelf in engraving on glaſs by means of the fluor acid, and having obſerved that ſome pieces of the glaſs I uſed were more ſuſceptible of being attacked by it than others, I reſolved to try the effects of this acid on different kinds of vitreous ſtones. Conceiving that the figures would be corroded with more neatneſs, the leſs the ſiliceous earth contained in the ſtones was mixed with foreign matter, I expoſed a very bright piece of rock-cryſtal, from Swiſſerland, at a temperature of 18° Reaum. for twenty-four hours, to the vapour of the fluor acid; and found, contrary to my expectation, that no impreſſion was made upon it. I repeated the experiment, doubling the temperature and the time of expoſure; but the ſtone did not loſe any of its ſplendor. It is well known that pure rock-cryſtal

* From *Voigt's Magazin für den neueſten zuſtand der naturkunde*, Vol. I. Part 3.

differs

differs from the coloured kinds by this circumſtance—that by expoſure to heat, the former undergoes no change, while the greater part of the latter do. It is equally well known that the action of the fluor acid on glaſs is increaſed by heat.

In order that I might begin a ſeries of comparative experiments in this reſpect, I expoſed, for thirty-ſix hours, at a temperature of 40°, a ruby, ſapphire, leuco-ſapphire, emerald, Oriental garnet, amethyſt, chryſolite, avanturine, giraſol, a Braſilian topaz burnt, a Saxon topaz raw, and an opal; but after being taken from the apparatus, they ſeemed as little injured as the pure tranſparent rock-cryſtal of the firſt experiment.

The diamond, which by its combuſtibility ſhews itſelf to be a peculiar genus, did not ſuffer the leaſt change after four days expoſure to the vapour of the fluor acid; the apparatus being placed on a common German ſtove.

Poliſhed granite, being expoſed to the vapour for three days on a ſtove as before, neither the quartz nor the mica ſeemed to have been attacked. The feld-ſpar, however, attracted my attention; being opaque and muddy, and covered with a white powder. I therefore repeated the experiment on a thin fragment of feld-ſpar of a reddiſh colour, noting its weight, which was 38 grains; and found it to be $2\frac{1}{2}$ grains lighter. The ſtone had alſo become whitiſh and friable at the ſurface, exactly as when in a natural ſtate of efflorescence.

The different ſpecies of flints, mixed with foreign earths, are more or leſs fuſible. But as this difference is not exactly in proportion to the quantity of the earths mixed with them, the proper explanation of the phenomenon muſt be ſought for in the different degrees of affinity which theſe kinds of earth have for calorie.

Figures traced out on the following ſtones through a covering of wax, after being expoſed for twenty-four hours to the vapour of the fluor acid, the apparatus being placed on the ſtove, were all found to be etched: Chryſopras, Hungarian opal, onyx, Perſian cornelian, agate, chalcedony, green Siberian jaſper, common flint. On the chryſopras the corroſion was above half a line in depth. In thoſe places the

green

green colour of the ſtone had diſappeared, and the cavities were filled with a white powder. The ſtrokes of the figures were expreſſed with the greateſt fineneſs and regularity on the opal; and the lines, in the like manner, were filled with a white powder. The onyx exhibited the contours very clearly, and the etching was pretty deep: the powder with which they were filled was likewiſe white. On the cornelian the figure was in part etched, and filled with white powder; in part there was only a white efflorescence, though ſtill compact and entire. The agate and chalcedony were, on the other hand, corroded white, but very unequally: here and there cavities were formed, each of which was lined with a white compact ſubſtance. The green jaſper was corroded very unequally, but almoſt to as great depth as the chryſopras. Some parts which remained compact, and which were only as it were efflorescced, had loſt their green colour and become white.

Flint. The uncovered part, of a bright brown and ſomewhat tranſparent ſpecimen, had become totally white, but was nevertheleſs compact. As I had covered the ſtone with wax, leaving a ſmall ſpot bare, without delineating any regular figure, I obſerved that the efflorescence had begun at the edge of the wax, and proceeded thence to the centre in ſuch a manner that the white contour thereby produced reſembled an imperfect figure of a fortification, while the inner ſpace was only partially efflorescced, and ſtill grey, but interſperſed here and there with white points. To free the ſtone from the wax, I waſhed it in ſpirit of wine. The white figure then gradually diſappeared: in half a minute nothing of it was to be ſeen; and the thin fragment ſeemed tranſparent as in its native ſtate. When it became dry, however, the opaque white figure again made its appearance. After moiſtening and drying it ſeveral times in ſucceſſion the revival of the natural colour and opacity became always more perfect, and the white efflorescence remained viſible when wet. As all theſe effects were produced by water and other liquids as well as by ſpirit of wine, as I afterwards remarked on other ſpecimens, theſe phenomena of the flint had a great ſimilarity to the change produced in the colour of the

opal,

opal, and its becoming tranſparent in water. I found means to ſtop the action of the fluor acid on a cornelian and a dark brown jaſper, at that ſtage of the proceſs when the ſtone was ſtill compact, and the colours only whitened. In water both theſe ſtones recovered their natural colour, and when dry became again white.

I expoſed another opaque and almoſt black piece of flint, with effloreſced white points, and the uſual white cruſt, without any covering of wax, at the uſual ſtove-heat of about 16°, to the vapour of the fluor acid for five days in my apparatus. At the end of that period it had loſt almoſt 1-8th of its weight; for it was reduced from 103 to 91 grains, and was thoroughly white, ſo that the ſound parts of the nucleus had entirely the appearance of the effloreſced cruſt. Some parts were friable; and I at the ſame time remarked that the parts of the nucleus, already naturally effloreſced, as well as the cruſt, were much leſs acted upon than the ſound black parts.

It is found alſo, that in regard to the natural efflorescence of ſtints, the effloreſced cruſt ſerves the ſound nucleus as a cover to defend it, and, by theſe means, retards the total decompoſition of the ſtone. Some, from this cruſt being formed of ſeemingly foreign materials, which is ſometimes conſidered as lime and ſometimes as clay, and which in common reſembles both, have deduced too much, in regard to the formation of ſtints, when they explain the phenomenon by ſeparation and ſucceſſive hardening from one of theſe kinds of earth. To judge from the above experiments, this cruſt is not properly the matrix, but a reſult from the decompoſition of the ſtone itſelf.

The efflorescence of feld-ſpar into a granite-like mixture is, with juſtice, conſidered as a very remarkable geological phenomenon, as it announces an endleſs ſeries of the total decompoſition of the preſent combinations. The ſucceſſive degrees of the decompoſition, by means of theſe experiments, may be exhibited with a ſmall and very ſimple apparatus. Turmalin from the iſland of Ceylon, and Zillerthal in Tyrol, green and black columnar ſchorl, and olive-coloured hornblende, after twenty-four hours expoſure, had experienced no change.

White

White Carara marble, in a temperature of 20°, loſt, in twenty-four hours, $\frac{1}{20}$ of its weight; but ſtill the ſhining ſurface of its cryſtalliſed texture was diſtinguiſhable. Weak ſulphureous acid diſſolved one-fourth grain out of forty-five; while, of another fragment, of 18 grains, $1\frac{1}{2}$ grain was diſſolved in the ſame time. Black marble ſuffered no loſs, either in its colour or weight. Agate was not attacked.

Lamellated tranſparent gypſum fell into white powder on the ſurface, after being expoſed for a few hours in the apparatus to the common heat of the ſtove, and the loſs of its weight amounted to $\frac{1}{12}$. This powder was not ſoluble in diluted nitrous acid. From this it appears that the vapour of the fluor acid did not deſtroy the combination of the calcareous earth and the ſulphureous acid, but only abſtracted the gypſum from its water of cryſtalliſation.

As the zeolite, among the ſiliceous ſtones, contains the greateſt quantity of the water of cryſtalliſation, as the gypſum does among the calcareous, I expoſed 102 grains of ſtriated zeolite at the ſtove temperature. In forty-eight hours I found its ſurface friable, and its weight only $85\frac{1}{2}$ grains; conſequently $\frac{1}{6}$ leſs. When immerſed in water, and again dried, it had increaſed $2\frac{1}{2}$. It now weighed 88 grains, but did not recover its ſplendour. On the tin plate, to which the ſtone had been faſtened with wax, I obſerved near the latter a white powder, which had the appearance of ſomething volatiliſed from the ſtone. I poured over the frothy cake of ſparry fluor powder and ſulphurous acid a little water, and, after ſome hours, found the inner ſides of the apparatus of tin plate covered with a beautiful ſilky ſubſtance, of the brightneſs of mother-of-pearl, which was perfectly like that of the zeolite in its natural ſtate. But as I afterwards remarked the ſame appearance of a ſplendour like that of mother-of-pearl, after expoſing other ſtones in the ſame manner, it is not to be aſcribed to the zeolite excluſively. On the contrary, I rather conſider it as an imperfect calcination of the tin; as, by a quicker diſengagement of the fluor acid, it, or a mixture of it with ſulphurous acid, may acquire the property of attacking tin.

Barytes, of a fibrous texture, when expoſed twenty-four

hours in the ſtove-heat, remained unchanged, and had ſuſtained no loſs of its weight or its ſplendour.

That I might not leave magneſian ſtone unexamined, I expoſed, for forty-eight hours, at the ſtove-heat, a thin plate of Venetian talc weighing 124 grains. After the experiment it weighed only 81 grains, and had therefore decreaſed in weight more than $\frac{1}{3}$: it had alſo fallen into a ſoft tender powder, which floated on water, and had the appearance of magneſia. I poured water on the reſiduum in the apparatus, and found next day the ſides incruſted with ſmall cryſtalline glittering flakes, adhering in detached maſſes, which could not be waſhed off with diluted nitrous acid. Though theſe may have been real magneſian ſalt, they ſeem, however, to throw ſome light on the cryſtalliſation of ſtones. Bergman had before obſerved the ſiliceous cryſtalliſation formed in diluted fluor acid, which had ſtood a long time at reſt over powder of ſilex.

Among the various hypotheſes reſpecting the formation of granite, and ſtones in general, that which ſuppoſes the ancient ocean, ſo much ſpoken of by modern geologues, to have conſiſted of a fluid totally different from that of our ſea-water, and of which the latter may be only the reſiduum, is not the moſt improbable. In my experiments, about 50 grains of the powder of ſparry fluor, mixed with as much concentrated ſulphurous acid as was equal to the ſpace occupied by the powder, was put into a tin-plate box, and the latter into another of the content of about 20 ſquare inches; to the lid of which, of ſtrong tin-plate, I faſtened, with wax, the ſpecimens to be tried, and ſhut the whole ſo as to be air-tight. After expoſing various kinds of ſtones for fourteen days I found the inner ſmall veſſel in part corroded, and on the exterior ſides a moiſt, weakly-adhering ſalt, cryſtalliſed in a conſiderable quantity, which at firſt I conſidered as a mixture of iron and fluo-ſulphat of lead, tin being ſeldom worked pure, and the ſolder having begun to give way. It is, however, poſſible that ſome tin may have been diſſolved by the two acids combined. I diſſolved ſome of this vitriol in diſtilled water, and dropped into it ſome ſpiritous tincture of galls. The liquor became of a beautiful indigo-blue colour, inſtead of black or purple as I expected; and a very light

light precipitate, of the ſame colour, was depoſited at the bottom. A ſolution of muriate of barytes being dropped into the blue liquor, gave a white precipitate, like regenerated barytic ſpar, without changing the blue colour of the firſt light precipitate, which remained like a ſlimy ſubſtance floating on the other, nor the clear liquor: and the latter, after having ſtood ſeveral years, appears as deep a blue as can be produced by ammonia from a ſolution of copper. The blue ſlime was found fit for a pigment, but far inferior to Pruſſian blue.

On this occaſion I recollected the blue colour of lapis lazuli, which Marggraf aſcribes to iron, and in which Rinmann found fluor acid. This ſtone, after two days expoſure, remained unattacked, and its colour unchanged. If, however, it is claſſed among the family of the zeolites, it is, at any rate, of a changed nature; for the latter are eaſily acted upon by the vapour of fluor acid.

According to Bergmann, ſiliceous earth is not ſoluble in pure carbonic acid gas; but it is obſerved in common life, in places where ammoniacal gas as well as carbonic acid gas are diſengaged in abundance; ſuch as dunghills, greenhouſes, priſons, cattle-ſtalls, and ſoap manufactories, &c. that the glaſs in the windows becomes much ſooner opaque than in apartments where that is not the caſe. The cauſe of this is owing in part to a cruſt of foreign matter, and in part to actual corroſion. If it now be admitted that the carbonic acid gas has a conſiderable ſhare in producing the efflorescence of the glaſs in the above-mentioned places, we may aſſume this effect, under certain modifications, in regard to the natural efflorescence of ſtones; as it is not ſo much a ſolution of the earthy baſes, as a ſeparation of the cryſtalline connection.

Though analogical concluſions from chemical reſults are of little value, they have led me to the following conjecture: As the fluor acid hitherto, at leaſt as far as I know, has never yet been analyſed, and as its radical is unknown, I am of opinion that, till ſomething poſitive is learned, we may admit that fluor acid is not eſſentially different from carbonic acid but in the degree of oxygenation, and that they have both one radical, *viz.* carbon.

VIII. *An Eaſy and Cheap Method of preparing Sal Aëratus, (Carbonat of Pot-aſh.) By* E. A. Holyoke, *M. D. of Salem, Maſſachuſetts* *.

Sal Aëratus, or the ſalt formed of vegetable alkali ſaturated by fixed air (carbonic acid), is, on many accounts, ſo uſeful, that a communication of an eaſy method of preparing it at little or no expence may be beneficial. The following may therefore be acceptable, if it be not already commonly known. I have myſelf prepared this ſalt for ten or twelve years paſt in this way, and it is now kept in our apothecaries ſhops.

Take a large wooden box †, bore eight or ten holes, half an inch in diameter, in the ſide of it, juſt below the lower edge of the cover, at nearly equal diſtances all round; bore alſo as many holes in the circular bottom of the box, cloſe to the edge of it: then take another box of the ſame kind, but of a ſmaller diameter by half or three quarters of an inch; place this in the larger, and, to keep it ſteady, thruſt three or four wooden wedges between the two boxes. The two boxes ‡ being thus prepared, fill the inner one with the pureſt ſalt of tartar, or clean well-calcined pearl-aſhes, or any clean pure fixed vegetable alkali: put its cover on the outer box, leaving the inner one uncovered; ſling this double box, thus filled, with a cord, and ſuſpend it in a diſtiller's vat or ciſtern, while the waſh is fermenting, a little above the liquor, or in an empty ciſtern, if it has been much uſed, and ſtill retains the fixed air: let it remain in this ſituation for ſix weeks or two months, or longer if it is not wanted: let it then be taken out, and the ſalt, now fully ſaturated with the acid, be expoſed to the ſun and air to dry.

The ſalt thus prepared does neither efflorceſce nor deliqueſce

* From the *American Medical Repoſitory*.

† I make uſe of a common cylindrical box, about nine or ten inches in diameter, and between five and ſix inches deep.

‡ The deſign of the outer box is merely to prevent any duſt or dirt from getting into the ſalt, while the holes in it ſuffer the fixed air to be freely admitted.

in

in the open air, and, for all common purpoſes, is, I believe, equal to that prepared by cryſtalliſation.

Note. The pearl-aſhes had better be put into the box in moderate-ſized lumps than in powder, that the air may have free acceſs to it.

But if any chooſe to have this ſalt in its moſt perfect form, let him proceed in this manner:

Diſſolve as much of the clean vegetable alkali in boiling rain or other pure water as poſſible; filter the ſolution through paper, pour it into a jar of ſtone or earthen-ware, cover the veſſel in ſuch a manner as that the air may have acceſs to it, but ſo as to exclude all duſt or foreign matter. Let it be hung by a cord in a fermenting vat, or ciſtern, for a month or two, in which time a great many cryſtals will be formed; from which the ſuperfluous liquor may be poured off, and the ſalt dried in Hippocrates' ſleeve. The ſuperfluous liquor may be again ſaturated with more alkali, and again expoſed to the air in the ciſtern, without any loſs. This laſt is, without doubt, the moſt perfect mode of preparation, and I have ſometimes made uſe of it; but, as it is much more troubleſome to make than the other, and as the other, for all medicinal purpoſes, is perhaps equal to this, I have for the moſt part employed it.

This ſalt is much more tolerable to the palate, and may be taken in larger doſes than the naked alkali; and as it is decompounded by vegetable acids as well as the mineral, it may be exhibited inſtead of the alkali in perhaps every caſe where the latter is proper, unleſs the fixed air is judged improper.

It is much ſuperior to common alkali in forming Riverius's anti-emetic effervеſcing draught, as it contains a much larger proportion of fixed air (in which the principal virtue of that medicine is ſuppoſed to reſide) than the mildeſt fixed alkali, and is at the ſame time much more palatable.

I commonly direct about ʒ ii, or rather more, of this ſalt to be diſſolved in ʒ iii of fair water; a large ſpoonful of this ſolution, added to the ſame quantity of good vinegar, or lemon-juice, at the inſtant of ſwallowing it, makes an agreeable doſe. But the taſte of this ſolution is ſo mild, that, if

the preſcriber chooſes, a ſpoonful of it may be ſwallowed alone firſt, and as much vegetable acid immediately upon it, in which caſe none of the gas will be loſt.

When acidity abounds in the firſt paſſages, a little of this ſalt added to any bitter infuſion, or the dry ſalt added to powder of columbo, or any peptic powder, is an effectual antacid.

In calculous caſes this ſalt is recommended by writers, particularly by the celebrated Dr. Cullen in his Materia Medica, Vol. II. ch. 13. as being an happy expedient for conveying larger quantities of alkali into the ſtomach, than it can bear in its natural ſtate.

Hitherto the common mode of preparing the ſalt for this purpoſe, I believe, has been by impregnating a ſolution of fixed alkali with fixed air, by means of Dr. Nooth's machine; but any one who has prepared the medicine in both ways, will readily give the moſt decided preference to that above deſcribed, on account both of eaſe and cheapneſs.

It is ſcarcely worth mentioning, that, for œconomical purpoſes, ſuch as promoting fermentation in dough for bread or cakes, where pearl-aſhes is commonly employed, the ſal aëratus is much to be preferred, on account of the much larger quantity of fixed air eliminated in the proceſs.

IX. *Communication from* Capel Lofft, *Eſq. reſpecting the late Meteor and the preſent Comet.*

To the Editor of the Philoſophical Magazine.

SIR, *Troſton, near Bury, Suffolk.*

OBSERVING an account of the Meteor of Sunday Sept. 22, in p. 434 of your laſt Number, I trouble you with an account of it as obſerved here.

I was looking on that evening for the comet, which I expected to have ſeen between the Northern Crown and the conſtellation of Hercules. This being about half paſt eight, my eye conſequently was then directed weſtward. The ſky was cloudy and miſty: very few ſtars appeared. Suddenly my attention

tention was called off from its immediate object by a moſt vivid reflection of light from the clouds of that part of the ſky; as if full day had ſprung on me inſtantaneouſly. I immediately turned round to diſcover whence it proceeded, and ſaw a moſt luminous body, apparently equal (or larger) to the full moon when ſhe appears greateſt, but certainly very much brighter. It was of an exceedingly ſplendid gold colour, and round, except to the weſt, where it was of a ſtrong red, drawing off to purple, and its edge ill defined, and rather unequal. It was about 12 or 15° high, and almoſt exactly in the meridian. It ſeemed nearly ſtationary; but what little motion it had, tending to the horizon nearly at a right angle. In about three or four ſeconds it diſappeared, as if ſinking behind the clouds: I obſerved no ſparkles, nor any luminous train left behind it, nor any exploſion. It was ſeen by many at Bury, and was alſo ſeen at Norwich, and at Cromer on the coaſt north-eaſt of Norwich. Thirty-five minutes paſt eight was the time I minuted of its appearance: but I had not then corrected my watch by an obſervation of the ſun on the meridian, for many days preceding. It might be about eight or ten ſeconds (as I did not find it immediately, the reflection being very widely extended) between my firſt being ſtruck with the reflected light, and the diſappearance of the meteor. Near Norwich it was obſerved to throw out red ſparks, or globules, as in Kent; and was noticed to be of a very white light: and the different colour of its light may be naturally referred to different ſtates of combuſtion, and partly to different ſtrata of atmoſphere through which it paſſed.

I do not at preſent learn it has been ſeen any where much weſtward of the line which theſe ſeveral obſervations indicated. It ſeems to have been very low; and if more obſervations could be collected and compared, it would probably be found to have a very conſiderable parallax, and its altitude and magnitude might be determined, eſpecially if obſervations could be had eaſt and weſt of this line. But it does not ſeem to have been ſeen at London, Peterborough, Oxford, or Lincoln; or even at Cambridge, though ſo very little weſt of the places where it was ſeen. Yet it was ſcarcely

poſſible

poſſible to have been not ſeen by any perſon who was out, if within the limits of the ſenſible horizon which circumſcribed it. This ſeems a ſtrong preſumption that it muſt have been uncommonly low indeed. Very few ſtars being then viſible, it was not eaſy to come to much accuracy as to its apparent path.

Meteors have abounded lately. I ſaw two very brilliant, but ſmall ones, in one night, and within three minutes of time of each other; one between the Northern Crown and Bootes, and the other between the Crown and Hercules. The former appeared about the ſize and brightneſs of Venus. And on the 2d, at about twenty minutes paſt ten, another, which was in the field of my night-teleſcope with the comet: beſide many ſmaller, uſually called *ſhooting ſtars*.

The laſt time the weather has permitted me to ſee the comet, was October 4, at twenty-five minutes paſt ten, to a very few minutes of its ſetting. I thought its nucleus very diſcernible, and the extent of its coma rather increaſed. From its then appearance I ſhould hope, if the weather favours, it will be traced down to its node; which ſeems likely to be in 17 or 18 of Sagittarius; and that it may ſtill be viſible for a fortnight from this time.

I remain yours ſincerely,

CAPEL LOFFT.

P. S. It is worthy of inquiry and obſervation, whether nearly all the very large meteors have not been ſeen in or near the magnetic meridian, as an acute and attentive philoſophical obſerver thinks to be the fact.

X. *Deſcription of a Blaſt-Furnace for ſmelting Iron from the Ore, with that Part of the Blowing-Machine immediately connected with it. By Mr.* DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

FIGURE 1, (Plate I.) repreſents a blaſt-furnace, with part of the blowing-machine.

A, the regulating cylinder, eight feet diameter and eight

feet

feet high.—B, the floating piſton, loaded with weights proportionate to the power of the machine.—C, the valve, by which the air is paſſed from the pumping cylinder into the regulator: its length 26 inches, and breadth 11 inches.—D, the aperture by which the blaſt is forced into the furnace. Diameter of this range of pipes 18 inches. The wider theſe pipes can with conveniency be uſed, the leſs is the friction, and the more powerful are the effects of the blaſt.—E, the blowing or pumping cylinder, ſix feet diameter, nine feet high: travel of the piſton in this cylinder from five to ſeven feet *per* ſtroke.—F, the blowing piſton, and a view of one of the valves, of which there are ſometimes two, and ſometimes four, diſtributed over the ſurface of the piſton. The area of each is proportioned to the number of valves: commonly they are 12 + 16 inches.—G, a pile of ſolid ſtone building, on which the regulating cylinder reſts, and to which the flanch and tilts of the blowing cylinder are attached.—H, the ſafety-valve, or cock; by the ſimple turning of which the blaſt may be admitted to, or ſhut off from the furnace, and paſſed off to a collateral tube on the oppoſite ſide.—I, the tuyere, by which the blaſt enters the furnace. The end of the tapered pipe, which approaches the tuyere, receives ſmall pipes of various diameters, from two to three inches, called *noſe-pipes*. Theſe are applied at pleaſure, and as the ſtrength and velocity of the blaſt may require.—K, the bottom of the hearth, two feet ſquare.—L, the top of the hearth, two feet ſix inches ſquare.—KL, the height of the hearth ſix feet ſix inches.—L is alſo the bottom of the boſhes, which here terminate of the ſame ſize as the top of the hearth; only the former are round, and the latter ſquare.—M, the top of the boſhes, 12 feet diameter and eight feet of perpendicular height.—N, the top of the furnace, at which the materials are charged; commonly three feet diameter.—MN, the internal cavity of the furnace from the top of the boſhes upwards, 30 feet high.—NK, total height of the internal parts of the furnace, 44½ feet.—OO, the lining. This is done in the neateſt manner with fire-bricks made on purpoſe, 13 inches long and three inches thick.—PP, a vacancy which is left all round the outſide of the firſt lining, three inches broad, and which is beat full of coke-duſt. This ſpace is

allowed for any expanſion which might take place in conſequence of the ſwelling of the materials by heat when deſcending to the bottom of the furnace.—QQ, the ſecond lining, ſimilar to the firſt.—R, a caſt iron lintel, on which the bottom of the arch is ſupported.—RS, the riſe of the arch.—ST, height of the arch; on the outſide 14 feet, and 18 feet wide.—VV, the extremes of the hearth, ten feet ſquare. This and the boſh-ſtones are always made from a coarſe gritted freeſtone, whoſe fracture preſents large rounded grains of quartz, connected by means of a cement leſs pure.

Figure 2 repreſents the foundation of the furnace, and a full view of the manner in which the falſe bottom is conſtructed.

AA, the bottom ſtones of the hearth. B, ſtratum of bedding ſand. CC, paſſages by which the vapour, which may be generated from the damps, are paſſed off. DD, pillars of brick. The letters in the horizontal view, of the ſame figure, correſpond to ſimilar letters in the dotted elevation.

Figure 3, AA, horizontal ſection of the diameter of the boſhes, the lining and vacancy for ſtuffing at M. C, view of the top of the hearth at L.

Figure 4, vertical ſide-ſection of the hearth and boſhes; ſhewing the tymp and dam-ſtones, and the tymp and dam-plates. *a*, the tymp-ſtone. *b*, the tymp-plate, which is wedged firmly to the ſtone, to keep it firm in caſe of ſplitting by the great heat.—*c*, dam-ſtone, which occupies the whole breadth of the bottom of the hearth, excepting about ſix inches, which, when the furnace is at work, is filled every caſt with ſtrong ſand. This ſtone is ſurmounted by an iron plate of conſiderable thickneſs, and of a peculiar ſhape *d*, and from this called the dam-plate. The top of the dam-ſtone and plate is two, three, or four inches under the level of the tuyere hole. The ſpace betwixt the bottom of the tymp and the dotted line is alſo rammed full of ſtrong ſand, and ſometimes fire-clay. This is called the tymp-ſtopping, and prevents any part of the blaſt from being unneceſſarily expended.

The ſquare of the baſe of this blaſt-furnace is 38 feet; the extreme height from the falſe bottom to the top of the crater is 55 feet.

XI. *Extract*

XI. *Extract of a firſt Memoir to ſerve as a Natural, Chemical, and Medical Hiſtory of Human Urine; with ſome New Facts on its Analyſis and Spontaneous Alteration. By* C. Fourcroy *and* Vauquelin *.

THERE is no animal matter which has been ſubjected to more examination than urine, and there is none which has furniſhed more diſcoveries to chemiſts. They have, however, confined themſelves chiefly to an examination of phoſphats, which were for a long time called Fuſible Salts. Margraff, Pott, Schloſſer, Haupt, and Rouelle the younger, were almoſt excluſively occupied with them from the time of Boerhaave to Scheele. This great attention beſtowed on phoſphats aroſe from the intereſt inſpired by the diſcovery of the phoſphorus of urine, and the ideas which the alchemiſts had propagated reſpecting the ſingular properties of theſe ſalts. What phyſicians have done in regard to human urine has ſcarcely had any relation to its nature, and has ſupplied too much to the ridiculous pretenſions of empiriciſm. The diſcoveries of Scheele put an end to the incoherence between medical obſervations and the chemical labours on urine. When the acid, which forms the greater part of the urinary calculi, or the uric acid, was diſcovered, as well as the acid phoſphat of lime, the formation of theſe calculi, like thoſe of the depots of the precipitates of urine, became much eaſier to be comprehended than before; and it then became poſſible to eſtabliſh between the medical obſervations on urine and its nature, better aſcertained, that relation which ought conſtantly to have been the phyſician's guide, ſince by it alone he could be furniſhed with any exact light. The uſeful inferences which our colleague Berthollet has drawn from the examination of urine, and from its nature, more or leſs acid, in gouty affections, are at preſent well known.

A long ſeries of experiments, undertaken by C. Vauquelin and myſelf, on animal matters in general, preſented to us

* From the *Annales de Chimie*, No. 91.

long ago a number of new facts in regard to the urine of men and animals. Our new analyſis of urinary calculi induced us to reſume, in greater detail, an examination of human urine, the natural ſource of theſe concretions. The reſult of our labour on this liquor we communicated to the Inſtitute in the ſitting of the 11th of Frimaire this year; and I ſhall here give a ſhort account of it, ſufficient to make it known, and to prove how much light may be thrown, by an analyſis of this kind, on the phyſical nature of animals.

I. In this memoir the ſmell of human urine is firſt conſidered as a very diſtinct and characteriſtic property of this liquid. In urine well conſtituted, and when it iſſues from the bladder, it is neither the odour of ammonia, nor of an acid, nor of the violet: it is evidently aromatic, and depends entirely on a matter peculiar to urine, which makes it to be what it is, and without which it would not be urine.

II. The orange colour of urine is, no leſs than its odour, a property which excluſively belongs to it, and which is found in no other animal liquid. Being ſuſceptible of many ſhades, and exceedingly varied degrees in its intenſity, as was long ago obſerved by Bellini, it is indebted for this variation to the very variable proportion in the effect of the water and the colouring matter, and the latter is the ſame that gives to this liquid its aromatic odour. The darkeſt coloured urine, either naturally or by artificial evaporation, aſſumes all poſſible ſhades, like thoſe remarked under different circumſtances, by the addition of water only in various quantities. Thus the ſmelling and colouring matter of urine is very ſoluble in water.

III. The authors of the memoir, in treating of the acrid and ſtrong taſte of human urine, remark that this acridity is not merely that of the ſaline ſubſtances held in ſolution in this liquor, which only modify it, by giving it a ſaline taſte. The durable acridity of urine depends alſo on the matter which produces its ſmell and colour. It varies, therefore, like theſe two properties, which it follows in their intenſity or diminution. When phyſiologiſts aſcribed the ſavour of urine to theſe ſalts, they did not know, or did not

pay

pay attention to the ſmall quantity of them in proportion to the maſs of water in which they are diſſolved.

IV. The component materials of urine, the number of which is conſiderable, re-act on each other during the proceſs of analyſing them. The ſalts contained in it are modified, and change their nature: but the property, above all, of becoming alcaliſed, or of forming, by its ſpontaneous alteration, ammonia and carbonic acid, (a property which is developed in a few moments in an elevated temperature, and which has made it be conſidered as the moſt alcalefcent of all the animal humours,) is the ſource of the moſt ſingular changes in its nature. Inſtead of remaining acid, it then turns vegetable colours green; it produces an effervefcence with acids when poured over it; it changes its colour; it aſſumes a fetid ammoniacal ſmell; it depoſits precipitates and cryſtalliſed ſalts which it did not contain. This alteration begins ſometimes even in the reins, and carries with it a diſpoſition to form calculi, which it would not have formed without it. It depends entirely on the urinary matter, the cauſe of its odour, its colour, and its ſavour.

V. It follows, from the preceding conſideration, that the analyſis of urine, by the means at preſent employed, muſt have given many uncertain reſults; and that many errors, in this reſpect, muſt have been committed. The action of fire, which ſo ſpeedily and ſo eaſily alters the nature of bodies, changes both the proportions and the properties of its productions. Urine, therefore, muſt be examined at the moment when it iſſues from the body; the component parts of it muſt, as much as poſſible, be ſought for without employing fire; in examining it, re-agents ought to be uſed, which, as in the analyſis of mineral waters, may ſerve to aſcertain, at the moment of their mixture, the matters contained in that liquid. The phoſphoric, the uric, and the muriatic acids, with lime and ammonia, have already been ſhewn in it; but ſcience is as yet far from being in that advanced ſtate as to poſſeſs the number of re-agents neceſſary for this kind of analyſis, which ſtill requires many new reſearches.

VI. Our reſearches on the means of analyſis, carried much farther

farther than any before undertaken, have been attended with no other ſucceſs than to enable us to compare the phenomena they exhibited with thoſe of evaporation. Thus the tranſparency of turbid urine, and the concrete flakes precipitated during the evaporation of urine, have ſhewn us that the cauſtic alkalies, and the precipitate formed by the tanning principle in the liquid, belonged to the phoſphat of lime and a gelatinous animal matter. We have learned, by the ſame comparative proceſs, that even a very gentle heat formed in urine ammonia, which ſpeedily neutraliſed its acidity: that its colour embrowned by evaporation, and its abundant cryſtalliſation, on cooling, after it had been brought to the conſiſtence of ſyrup, depended on the concentration of the particular matter, the common ſource of its colour, odour, ſavour, and its other characteriſtic properties; that the fetid garlic ſmell, and the cryſtalline form, were two of its moſt prominent characters: and, in the laſt place, that as all urine evaporated in this manner forms itſelf into a maſs, there ought to be found in this maſs the conſtituent matters of urine, except the portion of ammonia formed and volatiliſed by the evaporating heat. This lamellated cryſtalline maſs, treated with alcohol, was almoſt entirely diſſolved, and nothing remained but a little of the grey ſaline ſubſtance which the water ſeparated from the phoſphat of ſoda and ammonia, and a little phoſphat of lime and uric acid, inſoluble in the liquid, but which was inſulated from the calcareous phoſphat by the ley of cauſtic alkali. Theſe ſalts, and this acid, made ſome milliemes only of the weight of the urine; while the matter diſſolved by the alcohol formed ſome centiemes. The latter was compoſed of a little muriat of ammonia, benzoic acid, and urinary matter more abundant than all the reſt. Such is the ſeries of our analytical proceſſes, analogous to thoſe employed on the reſidues of mineral waters, and by the help of which we have been able to ſeparate the conſtituent matters of human urine more exactly than had been done before.

VII. The diſtillation of urine, though conſidered as well known, preſented to us ſeveral remarkable facts. In a very gentle ſand-bath, freſh urine gives water very ammoniacal and

and cryſtalliſed carbonat of ammonia long before it is dry: the laſt portions of water obtained effervefce ſtrongly with acids, and become of a roſe-colour, not much ſuſceptible of changing in the open air. It ariſes from the carbonat of ammonia which the liquid product contains; and urine has a ſingular diſpoſition to form this ſalt in great abundance and with great eaſe, as is proved by all the means of analyſis applied to it. The native acid of the urine is then ſaturated; flakes of animal matter are depoſited, as well as earthy phoſphats and the uric acid. All theſe phenomena continue until the predominance of the carbonat of ammonia, which is formed, becomes very manifeſt. They take place at a continued temperature of 60°.

VIII. We never ſaw, with any exactneſs, but three facts in regard to the putrefaction, or ſpontaneous and ſeptic decompoſition of urine, *viz.* the horrible fetor by which it is accompanied; the formation of a great quantity of ammonia which characteriſes it; and the abundance as well as eaſy extraction of the fuſible ſalts or alkaline phoſphat which follows. C. Hallé has given an exceeding good deſcription of the ſucceſſive alterations which urine, left to itſelf, experiences, but he has not followed them in regard to the nature of their materials; the object of his reſearch was only to deſcribe the effects in their appearances. Urine contained in a cloſe veſſel becomes of a darker colour; turns brown, and even black; emits a fetid, ammoniacal odour; depoſits firſt a light cloud, which is gradually changed into mucous flakes more or leſs coloured. There are formed at its ſurface, or on the blackened cruſt by which it is covered, and on the ſides of the veſſel which contains it, cryſtals in needles or in regular priſms, or ſilky tufts. Urine is then ammoniacal inſtead of being acid. Diſtilled to a half, it gives a great deal of the fetid carbonat of ammonia: being carried ſtill farther, ſo as to become ſyrupy, it furniſhes acetite of ammonia: the thick part gives, by the addition of acids, a ſharp and acetous odour. This reſiduum of urine, when putrid and evaporated, does not, by the addition of the concentrated nitric acid, preſent thoſe white concrete and abundant cryſtals which ariſe in freſh urine evaporated to the ſame degree, and

and which belong to the urinary matter not changed. It was this matter which experienced the greateſt and moſt ſingular change by putrefaction; it was the focus and ſubject of it; and it gave birth, above all, to the carbonat of ammonia which replaced it, and to the acetous acid, which, as the phoſphoric and the uric, is found ſaturated by this kind of alcali. It appears, therefore, that in order to procure more of the phoſphat of ammonia, it is of advantage to employ putrid urine.

IX. An attentive and careful examination of the firſt phenomena of the putrid decompoſition of human urine, preſented to us reſults as intereſting at leaſt as that of urine entirely decompoſed. Theſe reſults are connected, above all, with the formation of urinary calculi, which were one of the moſt important objects of our labour. We were deſirous of adding to the well-known fact of the exiſtence of calculous matter in every kind of urine, anſwers to the following queſtions of ſo much importance:—Why are theſe matters more abundant? Why does a diſpoſition to calculus exiſt in ſome ſubjects and not in all, though the urine of all contains what may form them? Why are they formed ſometimes ſpeedily, and ſometimes ſlowly? What is the cauſe of the ſix-fold variety of the calculous matters, their mixture, and interruption? Though we were far from having found a ſolution to all theſe queſtions, we have at any rate been able to reſolve ſome.

The white priſmatic cryſtals, which are depoſited on the pellicles or the ſides of veſſels containing urine, do not ſhew themſelves till the liquid becomes ammoniacal. They increaſe in quantity and bulk for ſix or eight days: they are priſms of ſix planes, with pyramids having ſix faces, which are readily diſcovered to be ammoniaco-magneſian phoſphat, like that often found, under the ſparry form, at the ſurface of white calculi. This ſalt does not exiſt, or is not formed, but when there is an exceſs of ammonia in the urinary liquor. This ammonia, by ſaturating the free phoſphoric acid, ſeparates the gelatinous matter which gives birth to a mucous precipitate, and conſtitutes with that acid, and by uniting itſelf to the phoſphat of magneſia, the ammoniaco-magneſian

phoſphat, which depoſits itſelf in cryſtals. The uric acid is equally ſaturated, and preſents urate of ammonia; which ſometimes depoſits itſelf in the calculous matter with the triple phoſphat juſt mentioned. This triple phoſphat ſeparates itſelf in cryſtals of the liquor only becauſe it is leſs ſoluble than the two inſulated phoſphats. This ſalt, therefore, which does not exiſt quite formed in the urine, is the produce of its ammoniacal alteration.

As ammonia and the carbonic acid go on always increaſing when the uric and phoſphoric acids and the phoſphat of magneſia are ſaturated, there remains in the liquor carbonate of ammonia, which then renders ſyrup of violets green, and effervefces with acids. The acetous acid, which is formed at the ſame time, becomes ſaturated alſo with ammonia; ſo that the urine contains, at the ſame time, acetite and carbonate of ammonia. Theſe three ſubſtances, formed almoſt at the ſame time by the decompoſition of the urine, ammonia, the phoſphoric and the acetous acids, are the produce of the urinary matter, which ceaſes to be found in that liquid when altered by putrefaction.

X. This alteration of urine does not always take place in the ſame manner, and its decompoſition varies according to the diverſified nature of that liquor. Sometimes, in one individual, when the urine commonly preſents the beforementioned phenomena, the liquid, inſtead of emitting an ammoniacal odour, becomes covered with a green and white mouldineſs, which increaſes for fifteen or twenty days. Inſtead of containing ammonia naked, it contains the acid, and emits a ſmell of it. This kind of urine is leſs ſubject to alteration than the preceding. It appears to be at leaſt as common as that which alcaliſes. Their difference depends evidently on the variation of their principles, and not on their nature; for they are almoſt always the ſame, except in their proportion. Thus the urinary matter, the ſource of the formation of ammonia, of the carbonic and acetous acids, and the cauſe of the alterability of urine, does not produce or experience that alteration, or that decompoſition, but ſo far as it is mixed with a certain quantity of gelatinous matter, which ſerves it as a ferment. If it does not contain

enough, and if the urine, leſs gelatinous, is by theſe means more coloured, has a ſtronger ſmell, and is more charged with urinary matter, it is leſs ſuſceptible of fermentation or putridity; it preſerves itſelf much longer and with its primitive characters, and is more permanent: that, on the other hand, which is leſs coloured, more changeable, and more diſpoſed to the formation of ammonia, depoſits ſpeedily mucous flakes, and gives ſooner a cloud and precipitate. It appeared to us, that urine leſs corruptible and leſs gelatinous, and in ſome meaſure more urinous, if I may be allowed the expreſſion, was the ſign of good health, and the produce of complete digeſtion; while pale urine, more gelatinous and more decompoſable, exiſted more particularly in weak ſubjects, and in caſes where the digeſtive faculties were leſſened. There is reaſon to believe that theſe two different ſtates of urine, which exhibit it as two diſtinct or different liquids, will one day furniſh facts of great utility to the healing art; and that a ſolution of tan will ſupply the means of diſtinguiſhing them, and of determining their relation in regard to the nature and quantity of the precipitate which it will form in theſe liquids, compared in different ſubjects, or in the ſame at different periods.

XI. Scheele firſt announced the preſence of the benzoic acid in the urine of man, and particularly in that of children. Rouelle the younger had before ſuſpected it in that of the mammeferæ, though he durſt not give any opinion as to its nature. This acid is obtained by ſublimation, on heating the extract of urine in cloſe veſſels. It may be ſeparated ſtill better by evaporating urine to the conſiſtence of clear ſyrup, and pouring on it the muriatic acid, by which it is precipitated; becauſe that acid decompoſes the benzoat of ammonia that has been formed. It is by the latter proceſs that we have taught the method of extracting it from the urine of horſes and cows, and, above all, from the water of dunghills, in ſufficient abundance to ſubſtitute it in pharmacy for that of benzoin. In a word, this acid is the leaſt abundant, and perhaps the moſt variable of the matters in urine. It appeared to us to be between $\frac{1}{200}$ and $\frac{1}{100}$. There are ſome morbific circumſtances under which its quantity

increaſes

increaſes very ſenſibly, and often very ſpeedily. We had no opportunity of determining the difference of the proportion in the urine of adults, or that of children, in which Scheele ſays that it is much more abundant.

XII. The analyſis of urinary calculi, which firſt directed our attention to urine as the ſource of its concretions, induced us to examine whether the oxalic acid exiſted in this liquid. The oxalat of lime is, indeed, one of the moſt frequent matters of calculi; and we have found it in the proportion of a ſixth in the number of urinary ſtones we examined. None of the means which can be employed to diſcover the preſence of that acid, exhibited it to us in urine; while, on the other hand, the ſmalleſt quantity of the oxalic acid, which we poured into the liquid, gave us an abundant and very heavy oxalat of lime; which proved to us, that ſuch an acid could not remain diſſolved in the urine. Thus, when a mural or mulberry-formed calculous, compoſed of the oxalat of lime and an animal matter, by which it is agglutinated, ariſes in the urinary duct, its production takes place even at the moment of the formation of the oxalic acid. This acid muſt ariſe in the urine; and in that caſe there muſt be in it an unnatural and morbific foreign production. There is reaſon to believe that ſome kinds of urine, which come from the body white and turbid, are charged with this ſalt; and that the oxalat of lime, formed by a cauſe ſtill unknown, iſſues in this manner without producing calculi. It may be thence ſeen of how much importance it is to make a chemical examination of the urine of diſeaſed perſons, and to eſtabliſh a continued ſeries of experiments on this ſubject in ſome hoſpital ſet apart for theſe uſeful reſearches.

The caſe is the ſame with ſiliceous earth as with the oxalat of lime. We have never yet found it in urine. And its exiſtence in that liquid appears to be a rare caſe in pathology: of three hundred calculi, indeed, analyſed with care, two only were found containing ſilex.

XIII. This analyſis ſhews that ſeveral matters, hitherto unknown in urine, exiſt in it, *viz.* 1. Phoſphat of magneſia. 2. The urat of ammonia, which is formed at the time of the decompoſition of the urine. 3. Albumen and gelatinous

matter. 4. The oxalic acid, which is produced under ſome morbific circumſtances. 5. Siliceous earth, which is found in it only very rarely. The four firſt ſubſtances are conſtant in it; the other two are only accidental, and therefore we paſs them over till after the examination of calculi ariſing neceſſarily from urine, and of which the conſtituent matters have been diſſolved in that liquid. Another new fact obtained by our labour is, that the particular matter which gives to urine its diſtinguiſhing characters, is converted, by means of the fermentation excited by the preſence of the gelatinous body, into ammonia, carbonic acid, and acetous acid, and from theſe new products there ariſe in urine very remarkable changes. The analyſis of putrid urine differs, therefore, very much from that of freſh urine.

It reſults from theſe reſearches, by which human urine has become much better known than it was before, that it contains ten principal or conſtant ſubſtances, characteriſed in the following manner:

A. Muriat of ſoda, which cryſtalliſes in octaedra in evaporated urine.

B. Muriat of ammonia; the natural octaedral form of which is modified into a cube by its union with the particular urinary matter like that of the preceding, and paſſes through the ſame combination from the cube to the octaedron.

C. The acid phoſphat of lime, forming about the 700th part of urine, conſidered formerly as an earth, precipitating itſelf by the addition of alkalies, which take from it its exceſs of acid; carrying always with it a gelatinous matter which this acid held in ſolution, and rendering urine turbid at the moment when the ammonia is diſengaged.

D. Phoſphat of magneſia, decompoſed by alkalies, and giving its earth mixed with the phoſphat of lime which is depoſited; becoming a triple ſalt, and ſeparating itſelf in cryſtals by the ſpontaneous formation of ammonia.

E. The phoſphat of ſoda, effloreſcing in the air, and always united to the phoſphat of ammonia.

F. The phoſphat of ammonia, little abundant when the urine is freſh; increaſing much by decompoſition and the forma-

tion

tion of ammonia, and giving only phoſphorus when the whole fuſible ſalt of urine is heated with carbon.

G. The uric acid, named very improperly at firſt the *lithic acid*. It cryſtalliſes by the cooling of urine, and forms the red ſand which that liquid depoſits at the bottom of pots. It is more abundant in the urine of diſeaſed perſons. It may be diſſolved exceedingly well by the cauſtic alkali.

H. The benzoic acid, more abundant in the urine of children; eaſy to be obtained from urine evaporated to a ſyrup, mixed to a tenth with concentrated ſulphuric acid, and diſtilled.

I. Gelatinous matter and albumen, extremely variable in its proportion in the different kinds of urine; ſhewing itſelf in clouds in urine where ammonia is formed, in filaments in that over which alkali is poured, in flakes by the evaporation of urine; precipitating itſelf by the tanning principle, which ſerves to determine the proportion; occaſioning a ſpeedy putrefaction of urine, which contains it in abundance; appearing by their augmentation to be the firſt cauſe of the formation of calculi, and furniſhing them with the gluten which unites their moleculæ, and following, in their proportion, the energy or weakneſs of the digeſtive forces of the diſtribution of the chilous matter.

K. The ſpecial urinary matter, giving to urine its characteriſtic properties really conſtituting it; giving it its odour, colour, and ſavour—its alterability into ammonia, the carbonic and acetous acids, &c. It is the moſt abundant of the matters contained in urine; it forms alone the $\frac{1}{2}$ of its matters. It has been improperly conſidered by Rouelle the younger as a ſaponaceous extract. To it is owing the almoſt total cryſtalliſation of urine evaporated to the conſiſtence of ſyrup; the ſolid and cryſtalline form it aſſumes in that ſtate by the addition of concentrated nitric acid; the cryſtalliſation of the muriates of ſoda and ammonia modified, and in ſome meaſure reverſed—the former from the cube to the octaedron, and the ſecond from the octaedron to the cube. This particular animal matter, which we call *urée*, will form the object of another Memoir, deſtined to ſerve as a ſupplement to

the preſent. It is of much importance to animal phyſiology and the art of healing.

It appears that, beſides theſe ten ſubſtances, the real and conſtant materials of human urine, it contains ſometimes, but rarely and accidentally, ſulphat of ſoda, muriat of potaſh, ſulphat of lime, oxalat of lime and of ſilex: that ſome of theſe ſubſtances, and particularly the two latter, are only the rare productions of ſome particular and perhaps morbific diſpoſitions of urine. It will be of great utility to inquire into the circumſtances which have an influence on the exiſtence of theſe matters, which are foreign to the natural ſtate.

XIV. In characteriſing urine as a liquid very diſtinct from all others, the matter which I have called *urée* gives it, above all, the property of becoming, by the decompoſition which it occaſions, a liquor very different from what it was when it iſſued from the bladder, and a ſubſtance totally new. Fermented urine is changed in almoſt all its matters. The view of theſe changes, which terminates this Memoir, exhibits, as the moſt ſtriking reſult, the production and exiſtence in putrid urine of nine new matters, which do not exiſt in freſh or natural urine.

1. Ammonia in exceſs.
2. The phoſphoric acid ſaturated by this alkali.
3. The phoſphat of magneſia converted into ammoniaco-magneſian phoſphat.
4. The urat of ammonia.
5. The acetous acid united to ammonia.
6. The benzoic acid ſaturated with the ſame ammonia.
7. The muriat of ſoda become octaedral.
8. The muriat of ammonia become cubic.
9. The carbonat of ammonia.

We may ſtill add the precipitation of the albumen and gelatinous matter effected by the ammonia, and which accompanies that of the phoſphats; ſo that theſe ſalts, like the matter of bone, are ſuſceptible of giving carbon when heated.

Such is the general view of the facts contained in this firſt Memoir. They prove how many new and important reſults a pro-

a profound ſtudy of urine may preſent to thoſe who will beſtow on it that continued attention which it requires, and what influence ſuch phenomena may have on the progreſs of animal phyſiology. The authors promiſe to make known, in a ſecond Memoir, all the properties which diſtinguiſh the particular matter that characteriſes this liquid, which is the moſt abundant in it, and which they call *urée*, as has been already ſaid.

XII. *Extract from the Report reſpecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combuſtion of the Diamond.* *By* C. GUYTON *.

THAT the diamond is combuſtible, is a truth which Newton, in ſome meaſure, conjectured; which experience has fully confirmed; and reſpecting which it is no longer poſſible to entertain the leaſt doubt. But what is the nature of this combuſtibility? My firſt experiments, publiſhed in 1785, on the entire combuſtion in nitre in fuſion †, ſeemed to announce that the diamond burnt in it after the manner of coal, ſince it left an effervescent alkali; and this ſuſpicion acquired more reality, after the examination made by Lavoiſier of the gas remaining in the veſſels in which it had burnt, and which he found charged with carbonic acid. Mr. Tennant has ſince furniſhed us with a new proof of this important fact, by repeating the combuſtion of the diamond by nitre in a gold crucible, as I had propoſed, in order to obtain a reſiduum abſolutely free from all foreign matter ‡.

There were, however, ſtill ſufficient reaſons to induce us to diſbelieve that the diamond and carbon, or that the diamond and the carbon extracted from the carbonic acid by the noble experiment of Mr. Tennant, were the ſame ſubſtance. Independently of their external characters, ſo completely different, ſeveral obſervations, which I have already communicated to the Inſtitute, prove that their chemical characters

* From the *Annales de Chimie*, No. 91.

† *Opuſcles de Bergman*, French edition, Vol. XII. p. 124.

‡ Philoſophical Tranſactions 1797.

no

no leſs excluded this identity. Indeed if the diamond was pure carbon, why had it not the ſame affinities? Why does it not, like it, ſerve to make the oxigenated muriat of potaſh detonate; to deoxygenate ſulphur, arſenic, and phoſphorus; to deoxydate metals, which are ſufficiently fixed to undergo the degree of fire which determines its combination with oxygen? Why does it not form alſo carbures? Why does it not, like it, conduct the electric fluid? We know that the aggregation conceals ſometimes the affinities by counterbalancing their power, but not in operations where the bodies are ſufficiently fixed, and the temperature ſufficiently high, to render effectual the weakeſt attractions. Something remained, then, to be diſcovered to harmoniſe and make us comprehend facts in appearance ſo contrary *. I imagined that it was by attentive obſervation of what took place during the act of the combuſtion of the diamond, that we ſhould attempt to penetrate this ſecret of nature. The experiments I am about to deſcribe will, I hope, prove that my attempt has not been vain; that the explanation of phenomena, which have appeared to us the moſt incoherent, may hereafter be deduced from ſome circumſtances which were not obſerved nor even ſuſpected, and which have enabled us to make an important ſtep in the knowledge of the nature of the diamond, ſince we can indicate ſubſtances which approach much nearer to it than carbon.

Theſe experiments, begun in Thermidor year 5, were not terminated till the 11th of Fructidor year 6. Various accidents, which may be readily conceived, and the fewneſs of the days when the rays of the ſun are not interrupted by clouds or weakened by vapours, were the principal cauſes of this delay. I ſhall ſuppreſs the details of thoſe eſſays which did not lead to concluſive reſults; but I ſhall not neglect thoſe which furniſhed us with an opportunity of obſerving ſeveral times the ſame facts, and ſometimes in a more diſtinct manner; though, by the effect of ſome unforeſeen circumſtance, it was not poſſible to keep an account of them in

* C. Berthollet, in his Lectures at the Normal School, ſtill leaves it a doubt whether the diamond is cryſtallized carbon, or carbon combined with ſome other ſubſtance.

regard

regard to the correſpondence of the ingredients and the products. I muſt not omit to remark, that I had, as co-operators in theſe experiments, C. Clouet and Hachette. The journal of them was correctly kept by C. Deſormes, formerly a pupil of the Polytechnic School, and at preſent aſſiſtant-preparer for one of the courſes of chemiſtry. The moſt important phenomena were ſeen, at different ſittings, by ſeveral men of letters.

The Council of the Adminiſtration of the Polytechnic School, approving the object of the experiments, authoriſed me to diſpoſe of ſome of the diamonds in its cabinet *.

The firſt experiment was made on the 9th of Fructidor in the year 5. We placed upon the table of the mercurial pneumatic tub, a bell of flint glaſs 15·3 centimetres in diameter, and of the content of 5580 cubic centimetres. Near the tub was placed, on one ſide, a pneumatic machine, to exhauſt the common air from the bell by means of a bent tube, which roſe as far as the knob. On the other ſide was a water pneumatic tub, bearing a large receiver, having at its tubulure a cock which communicated with the inſide of the bell by a tube of bent glaſs, and riſing, in the like manner, to the hollow knob of the bell. On one of the edges of the mercurial tub was fixed a ſlide, bearing a kind of mandril, deſtined to receive a cylinder of hard wood moveable in every direction, terminated by a handle of iron, and ſerving to ſupport a cup made of the crucible earth of Valogne; ſo that this cup could be conveyed to every point of the interior part of the bell to preſent the diamond to the focus. This apparatus is repreſented by fig. 1. (Plate II.) Every thing being diſpoſed in this manner, we put into the cup

* The diamonds, which form part of its collection of minerals, were found in an Engliſh ſhip from Senegal, captured in the year 2. They were depoſited at the *Hotel des monnoies*, where they remained till the year 5, when, on the ſuggeſtion of our colleague Monge, one of the adminiſtrators, the miniſter of the finances, after cauſing it to be aſcertained that the greater part of them were unfit for being cut, and for that reaſon more valuable for inſtruction, ordered one third of them to be given to the Muſeum of Natural Hiſtory, another to the Cabinet of the School of Mines, and another to that of the Polytechnic School. The lot aſſigned to the laſt conſiſted of 26, weighing altogether 3·662 grammes.

an incomplete octaedral diamond, having the edges a little rounded, of a dirty water inclining to yellowish-grey, and weighing exactly 142 milligrammes. On the cup, the edge of which was ground flat, a cover was applied, attached to a thread tied round the lower part of the support.

The pump was made to act till the mercury in the bell rose within less than a millimetre of the orifice of the tubes of communication. The cock of the receiver of the hydropneumatic tub, which had been previously filled with oxygen gas obtained from the oxygenated muriat of pot-ash, was then opened; and the first portion of this gas which had passed into the bell was extracted by the pump, in order to exhaust as much as possible the remainder of common air. After this it was filled with the same gas to within 69 millimetres of its internal edge, and 51 of the external. It may readily be conceived that still a little air remained in the cup in which the diamond was placed, and which, during these experiments, had been shut by its cover; but its content not being altogether three cubic centimetres, this inconvenience was thought the smallest of those which were to be apprehended.

The diamond having been uncovered, we began, at ten minutes after one, to throw upon it the focus of the large lens belonging to the cabinet of the Polytechnic School, the diameter of which is 40·59 centimetres, and the focus 135·3. We were well aware of the necessity of heating the bell by degrees, to prevent its cracking. For this purpose we interposed, at first, green and blue coloured glass; but whether they acquired more heat, or resisted dilatation more, these glasses all speedily broke; and we were not able to accomplish our object but by covering with paper, for some moments, the part of the bell which received the luminous disk. When the paper was withdrawn, the mercury fell rapidly, 19 millimetres in the inside. The diamond, exposed to the focus for twenty minutes, did not inflame. It appeared at first mealy, but sensibly blackened at the surface when observed through the coloured glass while exposed to the focus *.

The

* This carbonaceous colour had been before observed by Lavoisier in his

The focus having been intercepted by an opaque body to examine more clofely the ſtate of the diamond, no alteration was remarked in it, except that it had aſſumed a yellowiſh ſhade, perfectly like that of tranſparent amber. The atmoſphere beginning to become charged with vapours, the experiment was diſcontinued, in order that we might reſume it at a more favourable moment. This moment occurred the next day the 10th, and was announced by a riſing of the thermometers; one of which, expoſed to the ſun in the open air, roſe to 40 degrees; and the other, expoſed to the ſun alſo under a bell-glaſs to compare the interior temperature, roſe to 44 degrees.

The focus was thrown upon the diamond at 28 minutes after 11. At 42 minutes after 11 the cone of light was intercepted, and we ſaw the diamond red, tranſparent, and ſurrounded with a faint radiation. When cooled, its edges appeared blunted: we obſerved in it a black point; but it had become white, and had loſt the yellow colour acquired the day before.

The experiment could not be reſumed till the 15th. We began, by noting the height of the interior column of the mercury, to calculate, according to the temperature and preſſure, the volume of the aëriform fluid remaining under the bell; and we judged that it had decreaſed about 173 cubic centimetres.

The rays of the ſun were bright and ſtrong; but the air ſo much agitated, that while the thermometer under the bell was at 44·5, that expoſed to the ſun in the open air did not riſe higher than 32°. There was a moment, however, when the luminous cone produced a ſlight ſcintillation on the ſurface of the diamond. An opaque body, immediately interpoſed, made it appear red; but more obſcure than on the 10th. It was alſo found white after cooling.

Being aſtoniſhed that the diamond, when inflamed, as on the 10th, did not maintain of itſelf the temperature neceſſary for its combuſtion, eſpecially in oxygen gas, as happens to

his experiments made on the diamond with the large lens of Trudaine. See *Dict. de Chemie de l'Encyclop. Method.* Vol. I. p. 741.

metallic combuſtibles*, we imagined it might reſult from its being too much in a maſs, or perhaps alſo too much inſulated from every other combuſtible which might contribute to this temperature: and that we might make an attempt to remove this obſtacle, we introduced into the ſame porcelain cup, and without deranging the apparatus, a ſmall cut diamond of the weight of eight milligrammes; but there was no appearance that the combuſtion was in the leaſt augmented; and this ſmall brilliant, inſtead of being more rapidly attacked by the heat, after having been two days expoſed to the action of the ſolar fire, capable of igniting obſcurely the large diamond placed cloſe to it, gave no ſigns of inflammation, and was taken from the apparatus without having experienced the ſlighteſt alteration, either in the poliſh of its ſurface, or the vivacity of its edges.

On the 23d of the ſame month we took the diamonds from the ſaucer, to examine, with care, that which had given manifeſt ſigns of a commencement of combuſtion. It weighed no more than 88 milligrammes; it had therefore loſt 54, about 0·38 of its weight. It ſtill retained its original octaedral form; but the angles were blunted, and the edges rounded. The ſurface was tarniſhed, and full of ſmall inequalities; which, obſerved with a magnifying glaſs, preſented cavities, ſalient points, and ſometimes parallel ſections of the laminæ. In ſeveral of the cavities we could plainly perceive a ſort of ſpecks inclining to grey; but what appeared worthy of moſt attention was, a pretty large hollow almoſt at the extremity of one of the quadrangular pyramids, which ſeemed to indicate the place where the ſolar focus had exerciſed, at the end of the operation, its greateſt intenſity; and where we diſtinguiſhed a blackiſh ſtripe, not terminated like a ſtroke formed by a foreign body, but on the contrary ſoftening itſelf off, and penetrating into the interior part of the maſs by degrading its colour.

* We had the more reaſon to be ſurpriſed at this phenomenon, as M. Landriani had announced that the diamond, inflamed by braſs wire, burnt like it in oxygen gas, and with the utmoſt brightneſs: he, indeed, excepted the Braſilian diamonds, which he was not able to inflame by theſe means. *Annales de Chimie*, Vol. XI. p. 156.

I thought

I thought it might be of ſome importance to preſerve the ſubject of theſe obſervations, and that it would not diſgrace the collection of the cabinet of the School, with an inſcription alluſive to the experiment to which it had been ſubjected. Another diamond was therefore deſtined to be put into the apparatus, in order that it might be there ſubjected to entire combuſtion. This diamond was alſo a pretty regular octaedron, of a much more beautiful water than the preceding, and weighing 200·1 milligrammes, 3·77 grains.

As the ſeaſon was already ſo far advanced that it left us no hope of a ſolar focus as ſtrong as that from which we had obtained ſo little effect with the lenſes we had employed, I was deſirous of terminating the experiment with the great lens of Tſchirhauſen; and the claſs granted me permiſſion to take it from their cabinet.

This lens, as is well known, is 86·6 centimetres (32 inches) in diameter, and 211·076 (73 inches) focus. We augmented its power ſtill more by catching the luminous cone with the ſmall lens of the cabinet of the Inſtitute, the diſk of which is 37·89 centimetres, and the focus 56·83, which in this poſition was ſhortened to 5·41 centimetres.

A firſt fitting gave ſcarcely any ſigns of a commencement of combuſtion. Next morning, the luminous diſk having fallen on one of the parts of the bell which was thickeſt, it occaſioned it to crack. It was therefore no longer poſſible to compare the volume of the gas before and after the operation, nor to diſtinguiſh and aſcertain the quantities of the products. We confined ourſelves to making lime-water paſs through the interior of the bell before the fiſſure had ſuffered a ſenſible quantity of common air to enter, and we obſerved that it was much troubled.

The diamond which had been laſt expoſed was noways changed at its ſurface: it had, however, loſt two decimilligrammes of its weight; which was verified by the ſame balance with which it had been weighed, and which is capable of marking, in a very ſenſible manner, theſe fractions of the milligramme. Thus we were obliged to adjourn the experiment till the next ſummer, in order to find a more favourable ſun, and to have time to provide a new apparatus.

[To be continued.]

XIII. *Account*

XIII. *Account of the Proceſſes uſed by Mr.* SHELDRAKE *to ſeparate the Mucilage from Linſeed Oil, and to diſſolve Copal in Spirit of Turpentine, and in Alcohol* *.

To ſeparate the Mucilage from Linſeed Oil.

I HAD read in ſome book, the title of which I do not recollect, that linſeed oil might be purified by ſhaking it with water, which would imbibe the impurities, and leave the oil more limpid. I tried this experiment by ſhaking linſeed oil with warm water; and was ſurpriſed to find they did not ſeparate, but remained united in the form of emulſion. I then boiled them together, and found their tendency to ſeparate diminiſh. As it is the known property of gums or mucilage to keep oil and water united in this ſtate, I was induced to ſuſpect the preſence of mucilage in linſeed oil.

I had read in Doſſie a method of preparing what he calls *fat oil*. It is effected by placing linſeed oil in a ſhallow veſſel expoſed to the heat of the ſun, and ſtirring it frequently: in a certain time it loſes its property of drying, thickens, and acquires a degree of tenacity that makes it proper for a ſize or cement for gilders, &c. A ſimilar ſubſtance is alluded to by Leonardo da Vinci.

Taking the exiſtence of mucilage in this oil for granted, I conjectured that the alteration, produced in its texture by Doſſie's proceſs, aroſe from the evaporation of ſome of its principles, and the more intimate union of the reſt in conſequence of that evaporation. To verify this conjecture, I tried the following experiment:

I filled a half-pint phial full of linſeed oil, corked and tied it ſecurely over with a bladder. This I expoſed to the heat of the ſun in ſummer, during the whole day: after it had remained a few days in this ſituation, the upper part of the

* From the *Tranſactions of the Society for the Encouragement of Arts*, &c. Vol. XVII. The preſent may be conſidered as a continuation of Mr. Sheldrake's paper on Painting in Oil, in a manner ſimilar to that practiſed in the ancient Venetian School, publiſhed in Vol. XVI.: for a copy of which ſee *Phil. Mag.* Vol. I.

phial

phial was covered with drops ſimilar to thoſe produced by holding a wet bottle to dry before the fire. I then ſhook it well, which made the contents look muddy, and ſet it to reſt again. After a time it became clear, and a portion of tranſparent liquor, like water, lay at the bottom. I then repeated the ſhaking and ſetting it to reſt, till no additional quantity of this fluid was ſeparated.

By trying this experiment repeatedly upon oils procured from different places, I found that ſome oils afforded much more mucilage than others. From ſome I ſeparated a third part of mucilage; from others, a pint would not afford more than a table-ſpoonful, and ſometimes leſs. Whether this difference in the reſult was radically in the oils, or from a difference in the proceſſes conducted by means ſo variable as the heat of the ſun, I am not able to aſcertain.

Upon trying the ſame experiment with nut and poppy oil, I found the ſame reſult, but in a different degree. The average quantity afforded by nut oil was, I believe, not more than a third part of the average of linſeed oil; and the average of the poppy oil was not ſo much as a ſixth. In ſome caſes, particularly of the poppy oil, I did not obtain any.

The colour of the oil always diminiſhed as the mucilage was abſtracted; but the mucilage was always as colourleſs as water. It is a queſtion I will not pretend to decide, whether the colour of theſe oils depends upon the preſence of the mucilage, or upon any other principle which is deſtroyed by the action of light. I have in ſome inſtances had the oil as colourleſs as water.

This decompoſition of the oils, if it may be ſo called, is curious, as proving the mucilage in them; but, as it is very troubleſome, may it not be adviſable to prefer thoſe which have naturally the leaſt mucilage in their compoſition?

After pouring the oils from the mucilage, I put ſeveral quantities of the latter together, and found them mix without any difficulty. I mixed this mucilage with water, and found it unite with it in any proportion without becoming turbid. I laid it upon plates of metal, expoſing them to dry in the ſun and before the fire, and, when dry, waſhed them with a ſponge and water: but it ſhewed no tendency to diſ-

ſolve; though, while in a liquid ſtate, it ſeemed to poſſeſs all the properties of a gum.

As the proceſſes by which I diſſolved the amber and copal to make the oil varniſhes, are to be found in many books, and as it will be better for artiſts to purchaſe than attempt to make thoſe varniſhes, it can ſcarcely be thought neceſſary to detail thoſe proceſſes here: but, as I believe the methods by which I diſſolved the copal in ſpirit of turpentine and ſpirit of wine are not known, I ſhall now ſubjoin them.

To diſſolve Copal in Spirit of Turpentine.

N. B. Whatever quantity is to be diſſolved, ſhould be put into a glaſs veſſel capable of containing at leaſt four times as much, and it ſhould be high in proportion to its breadth.

Reduce two ounces of copal to ſmall pieces, and put them into a proper veſſel. Mix a pint of ſpirit of turpentine with 1-8th of ſpirit of ſal-ammoniac; ſhake them well together; put them to the copal; cork the glaſs, and tie it over with a ſtring or wire, making a ſmall hole through the cork. Set the glaſs in a ſand-heat ſo regulated as to make the contents boil as quickly as poſſible, but ſo gently that the bubbles may be counted as they riſe from the bottom. The ſame heat muſt be kept up exactly till the ſolution is complete.

It requires the moſt accurate attention to ſucceed in this operation. After the ſpirits are mixed, they ſhould be put to the copal, and the neceſſary degree of heat be given as ſoon as poſſible. It ſhould likewiſe be kept up with the utmoſt regularity. If the heat abates, or if the ſpirits boil quicker than is directed, the ſolution will immediately ſtop, and it will afterwards be in vain to proceed with the ſame materials; but if properly managed, the ſpirit of ſal-ammoniac will be ſeen gradually to deſcend from the mixture and attack the copal, which ſwells and diſſolves, except a very ſmall quantity which remains undiſſolved.

It is of much conſequence that the veſſel ſhould not be opened till ſome time after it has been perfectly cold. It has twice happened to me, on uncorking the veſſel when it was not warm enough to affect the hand, that the whole of the contents were blown with violence againſt the cieling. It is

likewiſe

likewise important that the spirit of turpentine should be of the best quality. I have never succeeded with that which is sold at the colour-shops; but whenever I procured my spirits at Apothecaries' Hall, I have dissolved the copal, by the process I have described, without difficulty.

This varnish is of a rich deep colour when viewed in the bottle, but seems to give no colour to the pictures it is laid on: if left in the damp, it remains sacky, as it is called, a long time; but if kept in a warm room, or placed in the sun, it dries as well as any other turpentine varnish; and when dry, it appears to be as durable as any other solution of copal.

To dissolve Copal in Alcohol.

Dissolve half an ounce of camphire in a pint of alcohol; put it in a circulating glass, and add four ounces of copal in small pieces; set it in a sand-heat, so regulated that the bubbles may be counted as they rise from the bottom; and continue the same heat till the solution is completed.

Camphire acts more powerfully upon copal than any substance that I have tried. If copal is finely powdered, and a small quantity of dry camphire rubbed with it in the mortar, the whole becomes in a few minutes a tough coherent mass. The process above described will dissolve more copal than the menstruum will retain when cold. The most economical method will therefore be to set the vessel which contains the solution by for a few days; and when it is perfectly settled pour off the clear varnish, and leave the residuum for a future operation.

This is the brightest solution of copal that I have seen: it is an excellent varnish for pictures, and may perhaps be found to be an improvement in fine Japan works; as the stoves used in drying those articles may drive off the camphire entirely, and leave the copal pure and colourless on the work.

N. B. Copal will dissolve in spirit of turpentine, by the addition of camphire, with the same facility, but not in the same quantity, as in alcohol.

At the time I determined to lay the preceding papers before the Society, I conceived that the quick and certain

manner in which the vehicle dried, was one of its advantages. But as that circumſtance has been objected to, and in ſome caſes really is a diſadvantage, I have ſince endeavoured to remove that objection by the following proceſs:

Put a pint of nut or poppy oil into a large earthen veſſel; make it boil gently upon a ſlow fire; put in by degrees two ounces of ceruſe, and ſtir it continually till the whole is diſſolved.

Have ready a pint of the copal oil varniſh heated in a ſeparate veſſel; pour this by degrees into the hot oil, and ſtir them together till all the ſpirit of turpentine is diſſipated; let it then be ſet by till cold, when it will be fit for uſe.

It is obvious, that, as this is a compound of the copal varniſh with the leaſt exceptionable of the drying oils, it will partake of the properties of each of its component parts. It gives leſs brightneſs and durability to colours than the varniſh will, but more than oil: but as it certainly may be uſed in painting in the ſame manner as any other drying oil, and gives more brightneſs and durability to colours than they can derive from any other oil, it is not unreaſonable to ſuppoſe that it will prove an advantageous vehicle.

I have mentioned ſpecific quantities of the ingredients; but it is eaſy to ſee that the relative proportions may be varied according as it is required to dry faſter or ſlower. It muſt be remarked too, that whenever the mixture is to be made, both the ingredients ſhould be hot; becauſe, if either of them is cold, the mixture becomes turbid, and a part, often the whole of the copal, is precipitated: but this inconvenience is avoided by mixing and boiling them together, as I have directed. It muſt likewiſe be obſerved, that after ſome time a ſpontaneous alteration takes place, which diminiſhes, and at laſt deſtroys the drying quality of this mixture: it will therefore be adviſable to uſe it freſh, or at leaſt not to uſe it after it has been made more than a month or ſix weeks.

XIV. *Proceſs for producing the Lights in Stained Drawings.* *By Mr.* Francis Nicholson, *of Rippon, Yorkſhire**.

THE difficulty of preſerving the lights in ſtained drawings, with freedom and preciſion, is ſo univerſally felt by thoſe who cultivate that branch of the arts, the practice of which is every day growing more extenſive, that the ſtatement of this circumſtance alone is ſufficient for the introduction of the following proceſs, by which that difficulty is removed, and by which all the effect of body-colour may be obtained without any of its inconveniences or defects. It is applicable to every ſubject, to the richneſs of foliage, of rocks, or of foreground; and in ruins, their moſt picturesque appendages of hanging ſhrubs, weeds, &c. may be expreſſed by it with the utmoſt ſharpneſs, and with all the lightneſs and freedom of which body-colour or oil-painting are capable.

The principle of this proceſs conſiſts in covering the places where the touches of light are intended to be, with a compoſition not liable to be diſplaced by waſhing over it with the colour, and ſuch as may be afterwards removed by a fluid in which the colours uſed in water are not ſoluble.

This compoſition, or ſtopping mixture, is made by diſſolving bees-wax in oil of turpentine, in the proportion of one ounce of wax to five ounces of the oil; and, as near the time of uſing it as may be convenient, grind with the pallet-knife as much flake white, or white lead, in oil of turpentine, as may be wanted at one time; dilute it with the above ſolu-

* The Society for the Encouragement of Arts, &c. having laſt ſeſſion received from Mr. Nicholſon, of Rippon in Yorkſhire, a Drawing intended as a ſpecimen of the proceſs for producing the lights in ſtained drawings, by removing, after the ſhadows are waſhed in, the colour where the lights are required, giving by this means the effect of body-colour with greater clearneſs, and without any of its diſadvantages; and it appearing that Mr. Nicholſon's method of tinting drawings promiſes to be of uſe in the practice of drawing in water-colours, and produces a more ſpirited effect, the Society agreed to Mr Nicholſon's propoſal, and purchaſed from him, at the price of twenty guineas, the complete proceſs for performing the work, as communicated in the above paper.

tion until it will work freely with the pencil, and appear on the paper, when held between the eye and the light, to be opaque. It is neceſſary to obſerve this, or the firſt touches will not be ſufficiently viſible, after being waſhed over with the colours, to aſcertain the places of the ſecond. It is alſo neceſſary to uſe a frame inſtead of the drawing-board, or to paſte the paper on the frame of the drawing-board ſo as to remove the pannel; becauſe the firſt and ſecond touches muſt be put on with the drawing placed between the eye and the light, as they will be moſt viſible in that ſituation. On this frame paſte the paper wet, ſo as to dry firm: when quite dry, draw the outline, and proceed as follows:—

1ſt, With a fine ſmall hair pencil, and the ſtopping mixture, cover thoſe places where the clear whiteneſs of the paper may be wanted, except in the ſky: let it dry a few minutes; then wet the paper on both ſides, and while it is wet waſh the ſky. The ſhadows of the clouds, diſtances, and general breadths of ſhadow, muſt be put in with the grey tint; and over the places of the light, waſh the tints of the brighteſt light; thoſe will be generally yellow ochre or light red.

The light of the clouds may be preſerved ſharp by preſſing on that part a piece of tiſſue-paper previous to the waſhing of the ſky: this, by abſorbing the ſuperfluous moiſture, will prevent the colour from ſpreading farther than is deſired. Suffer the whole to be very dry; and,

2dly, Touch in with the ſtopping mixture, the ſharp and prominent parts of the brighteſt lights; let them dry a few minutes, then waſh over them with the tints of the next degree of light.

3dly, Stop with the mixture the ſecond order of touches, and waſh over them with the middle tints; ſtrengthen alſo at the ſame time the breadths of ſhadow.

4thly, Stop, with broad touches of the mixture, the places of the middle tint; uniting them to the former touches, and extending them ſo as to graduate the middle colours into the ſhadow: ſtrengthen the ſhadows, making them nearly as dark as they are intended to be, and let the whole be perfectly dry.

Then

Then take oil of turpentine, and with a ſponge, or hog's-hair pencil, waſh over the places where the mixture has been uſed, rubbing it with the bruſh until it be diſſolved: clear it away with a linen rag, and waſh it with more oil of turpentine ſo long as any white lead appears; then let it dry.

Warm the drawing; then with a ſoft bruſh and highly-rectified ſpirit of wine waſh the places where the oil of turpentine has been uſed, to clear away the remainder of it: rub the drawing lightly on the face, but ſponge it well on the back.

When dry, tint down the lights where it may be wanted; harmoniſe the colouring, and cut the ſhadows to effect, with ſtill darker tints as may be neceſſary.

If other touches of light ſhould afterwards be wanted in the ſhadowed parts, the colour may be eaſily removed by a pencil formed of ſponge, with water ſufficient to produce them with as much ſtrength as can be deſired; then ſtop them with the mixture; waſh the ſhadow over the touches, bringing it to the colour taken off; and, when dry, remove the mixture with the oil of turpentine and ſpirit of wine.

XV. *An Account of Mr.* Brown's *Travels through Egypt and Syria*, &c.

[Continued from page 414 of the laſt Volume.]

DURING three or four days enſuing, Mr. Brown ſuffered ſo violent a relapſe as to be unable to perform the common offices of life, and even to ſuppoſe that it was nearly at an end. The moment any ſymptoms of amendment appeared, he ſent word to the Melek that he wiſhed to be introduced to the Sultan, and then, as ſoon as poſſible, to be diſmiſſed. No reply was made to this meſſage; but the following day he came to the tent, with ſome of his attendants, and deſired to ſee the merchandiſe he had brought with him. As to part of the articles, conſiſting of wearing apparel, &c. ſuited to the great, our traveller readily conſented: but this was not ſufficient; the Melek inſiſted on ſeeing the contents of a ſmall cheſt, which chiefly contained articles uſeful to himſelf, but

not

not deſigned for ſale. There were alſo in it ſome Engliſh piſtols, which he intended to avail himſelf of as preſents at Sennaar, or wherever elſe he might be able to penetrate. Mr. Brown, therefore, poſitively refuſed to open the cheſt: the Melek threatened to have it broke open; and, as his attendants were proceeding to do ſo, Ali Hamad, the man who attended Mr. Brown, took the key from its concealment and opened the box. Every thing was pulled out and examined, and many ſmall articles appeared no more: the piſtols were reſerved to be taken by the Sultan, after a violent but fruitleſs altercation at the valuation made by his own ſervants; and Mr. Brown's teleſcopes, books of which they knew not the uſe, with his wearing apparel, &c. were graciouſly left him. The valuation was made the following day: the whole was eſtimated at thirty-eight head of ſlaves, being at the market price worth eighty, excluſively of a preſent of value for the Sultan. A pair of double-barrelled piſtols, ſilver mounted, which coſt in London twenty guineas, were valued at one ſlave; which can in general be purchaſed, by thoſe who are experienced in that traffic, for the value of fifteen piaſtres in Egyptian commodities. On this Mr. Brown exclaimed, that if they meant to plunder, and if bargain and ſale were not conducted in the country by the conſent of the parties, but by force, it would be better to take the whole gratis. No anſwer was made; but the day following two camels were brought him as a preſent.

The violent manner in which our traveller's property had been ſeized, and the general ill-treatment he received, had much augmented his diſorder. He had been fifteen days in the tent expoſed to great variations of temperature; and it being at the cloſe of the rainy ſeaſon, he could rarely obtain water to drink, though tormented with thirſt. He judged, therefore, that the only means of reſtoration were, to return to Cobbé, and avail himſelf of the ſhelter of a clay houſe and privacy, the want of which he had ſo ſenſibly felt. The Melek, being in poſſeſſion of the greater part of his property, having left him only as much as would ſupply the wants of a few months, did not ſeem very anxious about his ſtay. Mr. Brown hired therefore two Arabs, and with the camels given

to

to him, and the property that remained, arrived on the third day at the place from which he had come. In the intervals of his illness he visited the chief persons of the place; and as the eyes of the people became habituated to him, he found his situation growing somewhat more tolerable. Though idle during the course of the winter respecting the immediate objects of his voyage, he grew, of course, more familiar with the manners and particular dialect of the country; for the Arabic spoken in it differs materially from the vernacular idiom of Egypt.

The following summer (1794) Mr. Brown, having in some degree recovered his strength, determined to go and reside for a time near the Sultan, both to supplicate for redress of what he had already suffered, and to embrace any opportunity that might offer of pressing his request for permission to advance. On his arrival at El Fasher, his good friend Melek Mifellim being employed by his master in the south, he went under the protection of the Melek Ibrahim, one of the oldest persons in authority there, and took up his lodging in the house of a man named Musa. During Mr. Brown's stay at El Fasher, of three entire months, he was solicitous to attend regularly the levees of the Sultan, which were from six in the morning till ten; but could very rarely obtain admittance, and when he did he had no opportunity of speaking. After waiting in fruitless expectation at El Fasher, when the time of his departure was drawing near, an accident happened, which, though not of the most pleasing kind, contributed to make him noticed, and obtained for him at length an interview with the Sultan. One day, as he was reading in his hut, a female slave belonging to the house, a girl about fifteen, came to the door of it, when, from a whim of the moment, he seized the cloth that was round her waist, which dropped, and left her naked. Chance so determined that the owner of the slave passed at that time, and saw her. The man immediately threw his turban on the earth, and exclaimed—Ye believers in the Prophet hear me! Ye faithful avenge me! (with other similar expressions:) a Caffre has violated the property of a defendant

of Mohammed! When a number of people was collected around him, he related the ſuppoſed injury he had received in the ſtrongeſt terms, and exhorted them to take their arms and facrifice the Caffre. He had charged a carbine, and affected to come forwards to execute his threats, when ſome one of the company, who had advanced fartheſt and ſaw Mr. Brown, called out to the reſt that he was armed, and prepared to reſiſt. It was then agreed among the aſſembly that ſome method of puniſhment might be found which promiſed more ſecurity and profit to the complainant, and would be more formidable to the guilty. The man who acted as his broker was to take the ſlave as if ſhe had really been violated, and agreed to pay whatever her maſter ſhould charge as the price. The latter had the modeſty to aſk ten head of ſlaves; and if Mr. Brown carried the matter before the Cadi, which he ſuppoſed he would hardly venture to do, he had ſuborned witneſſes to prove that he had received of him property to that amount.

On Mr. Brown's removal from Cobbé to El Faſher, he had cauſed his ſmall remaining property, among which were a few articles of value, but many of much uſe to him, to be lodged in the houſe of Hoſſein, the owner of the ſlave, and his companion. On his return thither, which happened a few days after the accident, he claimed it; but they reſiſted, as they alleged, at the ſuit of his broker, and would not deliver it till the value of ten ſlaves ſhould be paid to him. Mr. Brown from the firſt conſidered their conduct as ſo violent that if it reached the ears of government the claim would unqueſtionably be abandoned; and, indeed, his adverſaries had reſted their expectations only on the timidity which they had been accuſtomed to obſerve in Chriſtians of the country, whoſe accuſation and condemnation are in fact the ſame. He had not neglected to give the tranſaction all the notoriety he could without having recourſe to public authority, and thoſe to whom he had applied were decidedly in his favour. He therefore now went to his adverſaries, Hoſſein and his companion, and in their preſence offered to Ali Hamad a promiſſory note for the value of ten ſlaves at the

market-

market-price on his arrival at Kahira. It was however refuſed; and his cheſt, containing ſome German dollars and other articles, was ſtill detained. The reſt was given up.

Mr. Brown had been told that the Sultan was appriſed of the tranſaction previouſly to his departure from El Faſher, and that he intended to grant him redreſs; but after waiting about fifteen days without hearing any thing farther of his intentions, being weary of ſuffering, he determined to return. He had been arrived but a ſhort time when a *fulganaway*, or meſſenger, came expreſs from Court, with orders for him to repair to El Faſher immediately. The object of the meſſage was kept a profound ſecret, nor could he diſcover whether it portended good or evil. He left Cobbé the ſame evening, and arrived at the end of his journey the day following about noon. He repaired as before to the Melek Ibrahim, who on the following day introduced him at the public audience. The Sultan, as he retired to the palace after it was over, ordered all the parties to appear. Being come within the inner court, he ſtopped the white mule on which he was mounted, and began a ſhort harangue, addreſſing himſelf to Hoſſein and Ali Hamad Mr. Brown's ſervant, in which he cenſured, in a rapid and energetic ſtyle, their conduct towards him:—"One," ſaid he, turning to Ali, "calls himſelf Wakil of the Frank; if he were a Sherif and a Muſlim, as he pretends, he would know that the law of the Prophet permits not a Muſlim to be Wakil to a Caffre: another calls himſelf his friend: both are agreed in robbing him of his property, and uſurping the authority of the laws. Henceforth I am his Wakil, and will protect him." He then ordered the parties to repair to the houſe of Muſa Wullad Jelfûn, Melek of the Jelabs, under whoſe appropriate juriſdiction are all foreign merchants. Mr. Brown here gives the following account of the manner in which he had been before received by the Sultan:

"On my firſt audience," ſays Mr. Brown, "I was too ill to make much obſervation. He was ſeated at a diſtance from me; the viſit was ſhort; and I had no opportunity of opening a converſation. He was placed on his ſeat (*cúrſi*) at the door of his tent. Some perſon had mentioned to him

my watch and a copy of Erpenius's grammar I had with me. He aſked to ſee both; but after caſting his eyes on each, he returned them. The preſent I had brought was ſhewn him; for which he thanked me, and roſe to retire.

"During the following ſummer the firſt time I got admiſſion to him he was holding a divan in the outer court. He was then mounted on a white mule with a ſcarlet *beniſh*, and had on his head a white turban; which however, together with part of his face, was covered with a thick muſlin. On his feet were yellow boots; and the ſaddle on which he was ſeated was of crimſon velvet, without any ornament of gold or ſilver. His ſword, which was broad and ſtraight, and adorned with a hilt of maſſy gold, was held horizontally in his right hand. A ſmall canopy of muſlin was ſupported over his head. Amid the noiſe and hurry of above a thouſand perſons, who were there aſſembled, I was unable to make myſelf heard, which the nature of my ſituation enabled me to attempt, though not exactly conformable to the etiquette of the court, that, almoſt to the excluſion of ſtrangers, had appropriated the divan to the troops, the Arabs, and others connected with the government.

"On another occaſion I contrived to gain admittance to the interior court by a bribe. The Sultan was hearing a cauſe of a private nature, the proceedings on which were only in the Furian language. He was ſeated on a kind of chair, which was covered with a Turkey carpet, and wore a red ſilk turban; his face was then uncovered; the imperial ſword was placed acroſs his knees; and his hands were engaged with a chaplet of red coral. Being near him, I fixed my eyes on him in order to have a perfect idea of his countenance; which, being ſhort-ſighted, I had hitherto ſcarcely found an opportunity of acquiring. He ſeemed evidently diſcompoſed at my having obſerved him thus, and the moment the cauſe was at an end he retired very abruptly. Some perſons to whom I afterwards remarked the circumſtance, ſeemed to think that his attendants had taught him to fear the magic of the Franks, to the operation of which the habit of taking likeneſſes is imagined by ſome of the Orientals to conduce. He is a man rather under the middle ſize, of a complexion

complexion aduſt or dry, with eyes full of fire, and features abounding in expreſſion: his beard is ſhort, but full; and his countenance, though perfectly black, materially differing from the negro: though fifty-five years of age, he poſſeſſes much alertneſs and activity.

"At another of my viſits I found him in the interior court, ſtanding with a long ſtaff tipped with ſilver in his right hand, on which he leaned, and the ſword in his left. He then had choſen to adorn his head with the folds of a red ſilk turban, compoſed of the ſame material as the weſtern Arabs uſe for cincture. The Melek Ibrahim preſented him, in my name, with a ſmall piece of ſilk and cotton of the manufacture of Damaſcus. He returned in anſwer, *Barak ulla fi!* May the bleſſing of God be on him! a phraſe in general uſe on receiving any favour; and inſtantly retired, without giving me time to urge the requeſt, of which I intended the offering ſhould be the precurſor. It is expected of all perſons, that, on coming to El Father, they ſhould bring with them a preſent of greater or leſs value according to the nature of the buſineſs in hand. It is no leſs uſual, before leaving the royal reſidence, to aſk permiſſion of the Sultan for that purpoſe. With this latter form, which was to me unpleaſant, I ſometimes complied, but more frequently omitted it. But on this occaſion, having been long reſident there, I thought fit to make a laſt effort to promote my deſign. The day preceding that which I had fixed for my return, happened to be a great public audience. I found the monarch ſeated on his throne (*cârſi*) under a lofty canopy, compoſed not of one material, but of various ſtuffs of Syrian and even Indian fabric, hung looſely on a light frame of wood, no two pieces of the ſame pattern. The place he ſat in was ſpread with ſmall Turkey carpets; the Meleks were ſeated at ſome diſtance on the right and left; and behind them a line of guards, with caps ornamented in front with a ſmall piece of copper and a black oſtrich feather. Each bore a ſpear in his hand, and a target of the hide of the hippopotamus on the oppoſite arm: their dreſs conſiſted only of a cotton ſhirt of the manufacture of the country. Behind the throne were fourteen or fifteen eunuchs, clothed indeed ſplendidly in habiliments of cloth

or ſilk, but clumſily adjuſted, without any regard to ſize or colour. The ſpace in front was filled with ſuitors and ſpectators to the number of more than fifteen hundred. A kind of hired encomiaſt ſtood on the monarch's left hand, crying out a *plein gorge* during the whole ceremony:—"See the buffalo, the offspring of the buffalo; a bull of bulls; the elephant of ſuperior ſtrength; the powerful Sultan Abd-el-rachmân-el-raſhîd! May God prolong thy life, O maſter! May God aſſiſt thee, and render thee victorious!" From this audience, and thoſe which preceded it, I was obliged to retire as I had come, without effecting any thing."

Mr. Brown's reception with Muſa Wullad Jelfûn was very different from that which he had experienced in the houſe of Miſellim or Ibrahim. His behaviour towards him was complacent, and he affected to ſeek opportunities of hearing his ſentiments on ſuch ſubjects as occurred. In obédience to the Sultan's command, Mr. Brown now gave in an exact ſtatement of the property he had loſt, and ſubſtantiated the proof by the ſtrongeſt circumſtantial evidence. With regard to the ſlave, the moſt complete redreſs was afforded him: the charge brought againſt him was judged abſolutely futile, and ſhe was reſtored to her maſter; while he, on the other hand, was compelled to give up the cheſt, &c. which had been violently with-held. The plunder which had fallen into the hands of his ſervant, and his accomplice, was not ſo eaſily reſtored. The Melek, tired of a gratuitous juſtice, began to think that a lucrative compoſition was more eligible. The offenders, who had been obſtinate in the firſt inſtance, ſeeing how the cauſe relative to the *jarea* (female ſlave) had been decided, thought proper to offer to the Melek marks of their gratitude for the lenity they expected from him; and the Sultan was unwilling to imagine that the ſufferings of a Caffre could fall heavy on himſelf at the day of final retribution. At length the Melek, who in reality was ſupreme arbiter of the conteſt, contented himſelf with giving Mr. Brown the intrinſic value, about four head of ſlaves, inſtead of twenty-four or twenty-five, which at firſt he had unequivocally declared due to him, and promiſed he ſhould receive. Thus the matter was terminated.

[To be concluded in the next Number.]

XVI. *De-*

XVI. *Description of the Paliorum Lacus, or Lake Palius, in the Valley of Noto in Sicily.* By M. Dolomieu *.

THIS small but very remarkable lake, situated at the distance of two miles west from the town of Palagonia, or of a mile from Mineo, is surrounded by volcanic mountains, and lies in a small plain somewhat hollow towards the middle. This plain is half surrounded by steep rocks, which give it the appearance of a monstrous crater, that has sunk down by some convulsion. The lake is placed in its centre, and, as it were, in the middle of a funnel. Its depth often varies, and consequently its circumference. In the winter time it may be about sixty or seventy fathoms in diameter, and about ten in depth; but in summer, when great drought prevails, it is often entirely dry. At the period when I saw it, which was in the month of May, it formed an oval about thirty fathoms in length and twenty in breadth: its depth was about five or six. It had a strong smell of Jew's pitch, or asphaltes, even at a considerable distance. The water in its colour inclined a little to green, and had an exceedingly nauseous and disagreeable taste. I was told that the water was often tepid; but it had, at the time when I examined it, the temperature of the atmosphere. In several parts of it I observed a violent bubbling, and particularly in four places near the middle. This bubbling was stronger at certain intervals, and the water was thrown up sometimes to the height of two or three feet, rising in this manner every five or six minutes. There are periods when this boiling is stronger or weaker; but on these occasions no other noise is heard than that produced by the motion of the water.

When the lake is dry, one may without danger examine its centre, where there are several deep holes. From these holes currents of air, somewhat warm, continually arise, and throw back sand and other bodies if put into them. It is this aëriform vapour which, when the bason is full, forces up the water as before described, and makes it to be covered

* From *Magazin für das neueste aus der Physik*, Vol. III.

with

with foam. It would, no doubt, be important to examine accurately the nature of this vapour; but though I was provided with the proper apparatus, I could not obtain my object, becauſe I durſt not venture to wade into the water in order to reach the places where the bubbling appeared, and which were at a conſiderable diſtance from the bank.

The mud at the bottom and on the bank, which has a black colour, is tenacious, and ſmells like pitch. A little petroleum has alſo been ſometimes found at the ſurface of the water. The whole ſoil of the ſmall plain conſiſts of black, tough, reſinous, inflammable earth. A few years ago ſome ſtraw huts in this neighbourhood having been ſet on fire, the fire was communicated to the ground, which burnt with a whitiſh dull flame, like that of the inflammable ſprings in Dauphiny, during ſeveral months, and was extinguiſhed with the greateſt difficulty, as the fire, when deſtroyed in one place, broke out in another. Since that period great care is taken not to kindle fire in the neighbourhood. From this phenomenon I am induced to think, that the air which riſes through the water of the lake, and which probably finds a paſſage through the ground, may be of an inflammable nature alſo; as the air of marſhes, which burns without any noiſe. The fertility of this ſmall plain is ſo great, that it produces a moſt abundant crop every year, without requiring much agricultural care. In walking over the ground a hollow noiſe is heard; which ſeems a proof of there being beneath the ſurface ſubterranean cavities, like thoſe of Solfatara near Puzzuolo. From theſe circumſtances there is reaſon to think that this place has been formed from the remains of a fallen crater, a part of which is ſtill ſeen in the ſurrounding mountains; and between this lake and that of Agnano near Naples, there is no other difference than the greater quantity of water in the one, and the more violent efflux of vapour in the other. Some aſſert that the vapour of this lake is mortal; and that no bird, or other animal, can be expoſed to it without being killed. The vapour alſo which ariſes from the ground is conſidered as of a ſuffocating nature, ſo that people who lie down on the earth, or only bend their bodies towards it, are expoſed to the greateſt danger; though one

may walk over it without the leaſt fear of any pernicious conſequences. All theſe phenomena have a great ſimilarity to thoſe of the Grotto del Cane, near the lake Agnano.

On the banks of the lake Palius there are found a great many ſmall cluſters of aſhes and flag, exactly like thoſe adhering to the ſides of the craters of Ætna and Veſuvius. The mountains and lava around this lake exhibit the moſt evident marks of their great antiquity, as in many places they are covered with calcareous ſtones, from which it may with certainty be inferred that they muſt have originated at a time when the preſent continent was not inhabited. In the time of Diodorus Siculus, however, the crater, which forms the preſent baſon of the lake, ſhewed traces of its internal inflammation: as we are informed by this hiſtorian, that flames burſt forth from this ſpot; that the water poſſeſſed a conſiderable degree of warmth; and that a horrid thundering noiſe was heard. Being extremely deſirous to know whether a connection actually exiſted between this volcano and that of Ætna, as they are at no great diſtance from each other, I enquired whether any affinity had ever been obſerved between the eruptions of Ætna and the before-deſcribed phenomena of the lake; and whether, during theſe eruptions, a ſtronger ebullition had taken place: but I was aſſured that no perſon had ever obſerved any thing of the kind.

The phenomena of this lake have at all times been attended with ſo many ſingularities as to give riſe to fables without number: at preſent it is ſaid to be inhabited by a fairy. In ancient times theſe phenomena were aſcribed to the ſupernatural power and influence of ſome deity. On this account a very celebrated temple, the remains of which I could not find, notwithſtanding all the trouble I took to diſcover them, was erected here to Jupiter and the nymph Thalia. People ſwore alſo by this lake, with the ſame reſpect and ſolemnity as by the river Styx. What aſtoniſhed the ancients, and what even at preſent excites the wonder of all thoſe who viſit this lake, is the inceſſant ebullition of the water, though the leaſt increaſe is not obſerved in it on the bank.

In the neighbouring mountains I found under the lava a reſinous

reſinous ſtrong-ſmelling ſubſtance in horizontal ſtrata, which could be eaſily divided into laminæ of from one to two inches in thickneſs; but, in other reſpects, the lava of this volcano exhibits nothing but what has been ſeen in the other extinguiſhed volcanoes in the neighbourhood. The Val di Noto is the only part of Sicily where traces of ancient volcanoes are found. Travellers, who imagined that they found ſome of the like kind in other provinces, were either miſtaken, or ſuffered themſelves to be deceived by falſe marks.

XVII. *Obſervations on the Nature of the Fog of* 1783. *By* M. De Lamanon, *Correſpondent of the Academy of Sciences at Paris* *.

WHEN this fog, which may be called an electric fog, began to appear, I was at Sallon de Crau, in Provence. In order to free my neighbours and countrymen from uneaſineſs as much as poſſible, I wrote a letter to M. Artaud, editor of the *Courier d'Avignon*, in which, after ſpeaking of the nature of the fog, I ſaid it would be deſtroyed by the ſtorms that would not fail to enſue. The event fully juſtified this kind of prediction. Having learned from the public papers that this phenomenon was not local, but almoſt general throughout Europe, I made new obſervations, and traverſed the higheſt Alps of Provence, Dauphiny, and Piedmont; and, during the courſe of my travels, collected information reſpecting the fog, and the effects of the thunder. But before I offer that explanation of the phenomenon, which appears to me moſt probable, let me endeavour to give an accurate deſcription of it.

I. *Nature and Effects of the Electric Fog.*

1. In almoſt all countries the fog was preceded by a ſtorm.

2. It began the ſame day at places very remote from each other; as Paris, Sallon, Turin, Padua, &c. where it appeared, for the firſt time, on the 18th of June. M. Senebier

* From the *Journal de Phyſique*.

wrote

wrote to the Count de Saluces, perpetual preſident of the Academy of Turin, that the fog was obſerved at Geneva on the 17th. I read in the *Affiches* of Dauphiny, that it began at Grenoble on the 21ſt.

3. A north wind prevailed in ſeveral places when the fog began; and in other places, where it began the ſame day, a ſouth wind prevailed. The fog, after having ceaſed, re-appeared in ſome places by the north wind; in others, by an eaſt, weſt, or ſouth wind.

4. The atmoſphere was not every where equally dry. At Sallon I obſerved that it did not make ſalts enter into deliqueſcence; did not raiſe the hygrometer; did not prevent evaporation from being abundant; and did not even tarniſh glaſs, which I expoſed to it. The ſalt-pits at Hyeres, in Provence, cryſtalliſed a fortnight ſooner than uſual by the effect of the fog. Meſſrs. Toaldo and Senebier obſerved, the one at Padua and the other at Geneva, that the hygrometer did not reach the point which denotes humidity. In the Champſaur of Dauphiny, and at Turin, the fog was ſometimes humid.

5. The ſun, which was never ſeen but through the fog, appeared very pale in the day-time; of a blood-red colour at riſing, and ſtill more ſo at ſetting.

6. At Sallon the fog ſometimes diffuſed a very diſagreeable ſmell, difficult to be determined, and which ſome believed to be ſulphureous. This bad ſmell was perceived in other places.

7. It was hurtful to the eyes. At Sallon, perſons whoſe lungs were weak, found diſagreeable effects from it. The inhabitants of the Champſaur informed me that ſeveral people in that neighbourhood had violent pains in the head; and that, in general, they partly loſt their appetite. The inhabitants of this valley are the greateſt eaters in France: ſtrangers, who reſide among them, and drink their waters, eat almoſt as much as they, and are not ſo nice as uſual in regard to the quality of their food. This I experienced in 1783.

8. In Lower Provence, Languedoc, and other places, the fog ripened the corn, and was favourable to the harveſt. The

peaſants beheld, with the greateſt ſatisfaction, the effect of the fog on their crops; and yet were afraid of it. In Upper Dauphiny, and at Turin, it blighted ſeveral fields of wheat, and rendered copper buttons green. In other places it dried the plants.

9. At Padua, Turin, Paris, Sallon, Grenoble, the barometer remained almoſt always ſtationary at the point which denotes its mean ſtate.

10. There were ſome days remarkably warm; but, in general, the months of June and July were almoſt every where leſs warm than uſual. That year there was no ſummer on the high mountains of Provence and Dauphiny, where the ſhepherds of the plains of Crau and Camargue tend annually numerous flocks.

11. There were every where ſtorms of rain; and after theſe ſtorms the fog ſometimes increaſed, but moſt frequently was diminiſhed.

12. During the whole time of the fog, an electric machine I had at Sallon emitted few or no ſparks. A philoſophic friend at Sorgues, near Avignon, wrote to me, that his electrometer always indicated a great deal of electricity in the atmoſphere.

13. On the 4th of July, at five in the morning, M. Nicolas, phyſician at Grenoble, and M. Plana, apothecary, took four meaſures of fog and mixed them with two meaſures of nitrous air: the abſorption was 1-4th, and nothing remained but a gas, in which a candle became extinguiſhed ſeveral times. Atmoſpheric air generally contains nearly 1-3d of pure air (oxygen gas), and 2-3ds of mephitic air (azotic gas). Fontana's eudiometer gave the ſame reſult on the 7th of July: of three hundred parts of atmoſpheric air, thirty-two were abſorbed. The air of the fog, mixed with inflammable (hydrogen) gas, did not prevent it from exploding when a lighted taper was preſented to the neck of the bottle in which it was contained.

14. Almoſt all thoſe who have ſpoken of this fog, ſay that it was low. When I was on the top of Mount Ventoux, however, nearly 1040 toiſes above the level of the ſea, I ſaw it far above me. M. Senebier ſays, in his letter to Count

de Saluces, that it was ſeen in the Alps at an elevation greater than that of Mount Salève, which riſes 601 toiſes above the ſea. On the 22d of September (1783) I aſcended the higheſt Alps of Dauphiny, to the height of 1660 toiſes above the ſea. (No one has yet been higher in Europe.) The ſhepherds, who ſerved me as guides, all aſſured me that this fog had however paſſed over theſe mountains.

15. The loweſt part of the fog was the thickeſt and drieſt. I aſſured myſelf of this by proceeding from the borders of the ſea to the ſummits of the higheſt mountains.

16. It is probable, according to every account, that this fog overſpread almoſt all Europe, the iſlands of the Mediterranean, and a part of Africa. It covered the whole Adriatic Sea, but extended only to the diſtance of 100 leagues on the ocean. It was properly a continental fog.

17. The thunder this year (1783) occaſioned great devaſtation. In Provence and Dauphiny alone it killed nearly ſixty perſons, and a great number of animals. I have found no inſtance of its falling and doing miſchief in places higher than 450 toiſes above the level of the ſea. It would be too tedious to inſert here the name of every place, which I noted down in my journal, where I learned that the thunder fell. It will be ſufficient to relate the moſt ſingular effects it produced, and ſuch as may add to our knowledge reſpecting the nature of theſe terrible meteors.

According to Pliny, Plutarch, Seneca, &c. the olive, fig-tree, and laurel, are never ſtruck by lightning. Porta adopted this opinion, which was that of all the ancients. Toaldo ſays alſo, it is only thoſe trees which contain reſin that can perhaps eſcape the danger of thunder; ſuch as the olive-tree, laurel, fir, and ſome others of the like kind. This, in all probability, is the foundation of the practice, common among the populace, of keeping in their houſes, and placing on the ſummits of ſteeples and at the corners of fields, branches of olive that have been bleſſed, and of burning them in their houſes during ſtorms. It is poſſible, and even probable, that trees of this kind may be leſs frequently ſtruck by lightning than thoſe which contain a great deal of aqueous juices; but I can aſſert, that theſe preſervatives of the ancients are not

equal to our conductors. On the 21ſt of June the lightning burnt the leaves of a fig-tree, and all the bark of an olive-tree, in the territory of Sallon; and I learned that the ſame thing had happened formerly, and that there even had been fig-trees and olive-trees ſplit and tore to pieces by thunder.

There is ſome kind of thunder, ſays Seneca, accompanied with a loud report, by which men fall dead, and ſome become ſtunned and loſe their ſenſes. In 1783 I ſaw ſeveral inſtances which confirm the truth of this obſervation. At Pelliſanne in particular, which is ſcarcely a league from Sallon, the thunder, attracted by an iron croſs, killed two perſons, and deprived ſeveral others, as it were, of their ſenſes. The ſame thing happened at Freiſſinouſe, in Dauphiny.

Thunder ſometimes, ſays Seneca, renders wine frozen and congealed. Of the two perſons killed by the thunder at Pelliſanne, one loſt immediately all his natural heat, and the body was found extremely cold. Thunder muſt ſometimes produce theſe effects by occaſioning a ſtrong and ſpeedy evaporation.

Bodies ſtruck by thunder, ſays Plutarch, do not corrupt; dogs and birds do not eat them. This may ſometimes be the caſe, but I know ſeveral examples of the contrary *. On the day of the fair of Villefranche (July 22d) the thunder killed a mule, which corrupted ſo ſoon that it was found neceſſary to remove it. About eight or nine years ago, ſeveral ſheep were killed by lightning on the mountain of Sederon in Provence; and the ſhepherds abſtained, for a long time, from approaching the place, on account of the bad ſmell which they emitted. On the 1ſt of July, about four in the afternoon, the lightning fell at Carpentras on the Lazaret, (a place where thoſe who died of the plague were formerly buried,) and ſet fire to the wood-work. The fire being communicated to ſeveral quintals of gunpowder, an explоſion took place, by which five perſons were killed and fifteen wounded. The ſame flaſh of lightning traverſed the convent of the Carmelites, and melted part of the ſciſſars of the prior: it killed alſo a cow, which was eaten without any bad conſequence.

* See *Mr. Achard's Experiments, in Philoſophical Magazine*, Vol. III. p. 11.

Towards

Towards the end of the month of June, the curé of Eſpinouſe, in Provence, ſtanding at the door of the church in order to exorciſe the thunder, was killed, as well as his maid-ſervant and his clerk. At Mane, in Provence, the bell-ringer was killed by it. On the 26th of Auguſt the lightning was attracted by the iron croſs on the church of Sigoger du Hai, in Dauphiny; and, going round it both on the inſide and outſide, frightened the ringer ſo much that he ſwore he would never return thither again in the time of a ſtorm. At Pernes, in the Comtat, it overturned a croſs. This croſs, the remains of which I ſaw, was of white calcareous ſtone, and faſtened by a bar of iron to a column of *coquilliere* ſtone * of a yellowiſh colour. The iron had diſappeared; a part of the column was ſhivered, and blocks of it carried to the diſtance of more than twenty-five paces.

At Aix the thunder dried up a beam, and left in one place only the fibrous part. At Freſſinouſe, in Dauphiny, it killed two oxen; deprived a labourer of his ſenſes, and carried away one of his toe-nails. I muſt here remark, that his ſhoes were ſhod with iron. At La Motte du Caiſe, in Provence, it entered at a window, where there was no iron, and went out at the chimney. At Claret it unſaddled an aſs without doing him any hurt, and carried the packſaddle to a conſiderable diſtance. In ſeveral places, and particularly at Banon in Provence, the lightning tore off the hair from the heads of ſeveral women. At Saint Criſtol, in Provence, it carried away the half of a girl's body. At Avignon it carried away the half of a cat. I had this fact from M. Sauvan, who on that day (June 21ſt) obſerved the ball on the ſteeple of the Grands Auguſtins of Avignon covered with a crown of light, which continued three quarters of an hour, and diſappeared at eleven at night.

On the 22d of July there fell a great quantity of hail at Saint Eſprit: it was of a very large ſize. A girl, ſtruck on the head by a grain of it, applied her hand to the place and found her head-dreſs on fire.

On the 21ſt of June aſcending thunder was ſeen at Sallon;

* A kind of ſtone mixed with abundance of ſhells.

like

like phenomena were obſerved the ſame day in ſeveral other places. In ſeveral villages through which I paſſed, I was ſhewn a great number of trees ſtripped of their bark by the thunder; and I remarked that the part of the bark, or wood, carried away, was almoſt always broad at the bottom of the tree, and narrow at the ſummit. It would ſeem as if the lightning had met with obſtacles, and that it had not the ſame force when it attacked the higher parts of the tree. In my opinion the thunder which produced theſe effects was all aſcending; and I often obſerved holes at the roots of the trees which had been deprived of their bark. I obſerved alſo that dry land had been leſs ſtruck with the lightning than moiſt land, which induces me to believe that the greater part of the thunder that year was aſcending.

II. *Thoughts on the Origin of the Electric Fog.*

Several philoſophers adopted the opinion of the populace, and conſidered this fog as a natural effect of the earthquake which laid waſte Sicily and a part of Calabria. Toaldo thought that all theſe exhalations were brought from Calabria and Sicily by the winds which blew from the ſouthward: but this reſpectable philoſopher, at the time when he wrote, did not know that the fog was almoſt general throughout Europe. Beſides, the earthquakes in Calabria and Sicily took place chiefly in February, and the fog did not appear till the middle of June; that is to ſay, till more than four months after. In my opinion, then, this fog was not occaſioned by the earthquakes of Calabria and Sicily; but the fog and theſe earthquakes, as well as thoſe which we are aſſured took place in Iceland, had a common cauſe, which produced different effects according as they were modified by circumſtances and the nature of the places. Let us now ſearch for this cauſe; we ſhall find it in the annals of meteorology, and nothing is neceſſary but to diſtinguiſh it.

I have ſaid, in another place *, that the conſtitution of the atmoſphere depends chiefly on the nature and form of the ground, and that the revolutions of the air are ſubject to the

* *Journal de Phyſique*, Mars 1782, p. 187.

revo-

revolutions of the earth, over which they have an influence in their turn. If we conſider the different bodies or ſubſtances of which that part of the earth known to us is compoſed, we ſhall ſee that, notwithſtanding their apparent reſt, they all obey, as we may ſay, an inteſtinal motion; and that this motion gives continual riſe to new compoſitions and combinations, the connection of which eſcapes us, but which nevertheleſs exiſt. The remains of animals and vegetables ſtill diſtinguiſhable, and which occupy ſo much room in our globe; the acids which attack them; the aëriform fluids diſengaged from them; the metals and pyrites brought to perfection, or decompoſed; the fires ſeparated, or collected; the fermentations and effervescences; in a word, the innumerable decompoſitions and recompoſitions of all the parts of the earth, furniſh abundance of ſubtle matter, which, by its levity, diſengages itſelf at the ſurface of the globe, cannot, in certain circumſtances, reſiſt the gravity of the air, and riſes ſometimes to the ſummit of the atmoſphere. On the other hand, the rains which fall, moiſt fogs, and ſeveral other cauſes, make a part of the water, which detaches itſelf from the air, to penetrate the earth, and to combine with the ſubſtance of theſe exhalations. It is afterwards, in part, attracted by the external heat, or repelled by the heat of the earth itſelf; but it never returns to the atmoſphere as pure as it was when it iſſued from it. It carries with it a great part of that ſubtle matter of which I have ſpoken; and it is this afterwards which produces thunder, and almoſt all fiery meteors. There is then a continual communication from the earth to the atmoſphere, and from the atmoſphere to the earth. The greater part of theſe exhalations, in general, is ſpecifically heavier than the atmoſpheric air; and, if they are not impregnated with a certain quantity of water, they cannot quit the earth. They muſt neceſſarily be there collected, and accumulate to a greater or leſs depth in the time of great droughts.

Let us now conſult our regiſters, and thoſe of different obſervers, and we ſhall ſee that there had prevailed, at leaſt for nine years, an extreme drought, not only in Europe, but alſo in Africa and America. This was announced year after

year

year by our domeſtic as well as foreign gazettes; and we may conclude ſo from the minute obſervations of the celebrated Van Swinden, and the tables publiſhed by Toaldo, Cotte, Beraud, Beguelin de Romily, &c. Sometimes, indeed, rain took place in one country or other; but in general, till the preceding winter, an extraordinary drought prevailed. It began about 1774, and in the month of June 1782 was extremely great in Italy and in our ſouthern provinces. We experienced at that period a ſuffocating heat; the earth, as we may ſay, ſeemed to be on fire, and, in the Plain of Camargue, ſcorched the feet of the reapers to ſuch a degree that they were obliged at length to walk upon ſtraw: ſeveral died of heat with the ſickles in their hands, and there were a great many ſick. In a word, the drought and heat were ſo exceſſive, that, at two leagues from Sallon, the ſpiders, which in general are not venomous, occaſioned by their bite violent diſeaſes, which had a great affinity to thoſe occaſioned by the bite of the tarantula.

In conſequence of this great drought, the exhalations of the earth, ſpecifically heavier than the air, and deprived of that humidity which ſerves them as a vehicle, remained in the boſom of the earth, where they muſt have formed immenſe accumulations. The winter of 1782—1783 was rainy, particularly in Calabria and Sicily; and the Alps were covered with a great deal of ſnow. The ſpring alſo was in general rainy. The water then being filtered into the bowels of the earth, was at firſt abſorbed by the very dry exhalations there confined. This humidity, added to the warmth of the ſpring, no doubt occaſioned effervеſcences and fermentations; ſo that the exhalations, diſengaging themſelves with violence, in certain places convulſed the earth, as was the caſe in Calabria and Sicily. In proportion as the water filtered into the earth by its own weight, it found new exhalations, which, by diſengaging themſelves, occaſioned new convulſions, but leſs conſiderable on account of the leſs abundance of theſe exhalations. In places where they were heated by their mixture, they liquefied ſtones, and threw up volcanic iſlands, as in Iceland. In the laſt place, theſe ſubtle exhalations riſing into the atmoſphere from all parts, with the vapours, which

which ſerved them as a vehicle, did not at firſt alter its purity, being intimately connected with it; but they nevertheleſs exiſted, and produced in its higheſt regions the multiplied halos, paraſelena and parhelia obſerved that year. The heat increaſing, and the earth continuing to furniſh exhalations in proportion to the preceding humidity, theſe were communicated to the atmoſphere in a manner almoſt inſenſible: but the atmoſphere being at length ſaturated, theſe exhalations underwent new decompoſitions; ſtorms were formed, the atmoſphere was cooled, and ſuffered to eſcape a part of theſe exhalations, which fell again towards the earth, and in one day Europe was covered with a dry fog *. Local circumſtances, in regard to moiſture, winds, and clouds, exempted certain places from it for ſome time. The earth, however, continuing to furniſh exhalations, and theſe being united to thoſe which the atmoſphere, as we may ſay, had depoſited, deſcending and aſcending thunder were ſeen till the exhalations of the earth and the atmoſphere were conſumed. The atmoſphere being gradually purified, and the ſource of the exhalations exhauſted, the earth ceaſed to be convulſed.

XVIII. *Report on the Converſion of Soft Iron into Caſt Steel by means of the Diamond. Read in the French National Inſtitute, Thermidor 26, Year 7.* By C. GUYTON †.

THE Claſs will recollect the account which I gave of the grand experiment of the combuſtion of the diamond in oxygen gas in the focus of the lens of Tſchirnhauſen, and the new facts which I thence deduced reſpecting the true nature of the diamond; plumbago, which is its oxyd in the firſt degree; carbon, which is its oxyd in the ſecond degree; and the carbonic acid, which is the produce of its complete oxy-

* The ſame fog, I preſume, took place in America, where there had been great complaint of drought for eight years. It was not ſeen in the open ſea, becauſe it was abſorbed by the water: for this reaſon it did not appear in countries where the ſky was overſpread with clouds.

† From the *Annales de Chymie*, No. 92.

genation. They ſuggeſted to our brother, C. Clouet, the idea of ſearching for a confirmation of a new kind, by trying to make ſoft iron paſs to the ſtate of ſteel by cementation with the diamond.

It has hitherto been conſidered as certain, that iron does not melt but by paſſing to the ſtate of ſteel or caſt iron. But in what ſtate does the carbon enter into that combination? It might be conjectured, that it is in the ſtate of plumbago, or oxyd of the firſt degree; ſince that which is ſeparated by acids exhibits the brilliant blackneſs and incombuſtibility which form its principal characters. Hence ſome were inclined to conclude, that the carbon entered into this union in the ſtate of an oxidule; that conſequently the carbon employed in the cementation of ſteel began by deoxydating itſelf to a certain degree. This was even in ſome meaſure proved; as the carbon employed for this operation was indeed found to have a more brilliant aſpect, and nearly reſiſted incineration, like carbon in a maſs burnt in cloſe veſſels. But if carbon really burns in the cementation of iron, it ought to diſengage from it oxygen gas. This is a queſtion which I have endeavoured to reſolve by experiment.

I cemented ſmall bits of iron in a porcelain retort, which in the preceding operation had received a vitreous coating, and which conſequently was no longer permeable to air. Theſe fragments were all ſurrounded, on every ſide, by charcoal of beech pulveriſed, and very dry. The retort was put into the reverberating furnace, and a tube connected to it and carried under a receiver filled with mercury. There was diſengaged a quantity of elaſtic fluid, compoſed of carbonated hydrogenous gas and carbonic acid gas, the laſt of which was at firſt only 0·11 in bulk; towards the middle of the experiment, 0·13; and at the end, 0·15.

The converſion of iron into ſteel being found only little advanced, after three hours and a half expoſure to the fire, we put the ſame iron and the ſame carbon again into the retort, and expoſed it to the heat of a three-blaſt-furnace. This time there was only a very ſmall quantity of gas; but it was ſtill carbonated hydrogenous gas mixed with carbonic acid gas, and always with the ſame progreſſion of the latter; which

which made at firſt only 0·07 of volume, while the laſt portions contained 0·12. The iron on this occaſion was converted into ſteel, and even the fragments had united by a commencement of fuſion.

It was very probable that a part of the carbonic acid, collected in this operation, might have been formed at the expence of the remaining carbon and with diſengaged oxygen; but the conſtant preſence of the hydrogen only ſerved to indicate the difficulty of freeing the carbon entirely from the laſt portion of water it contained. I ſhall here take occaſion to obſerve, that this experiment ſeems not at all reconcileable with the opinion of ſome chemiſts, that hydrogen has more affinity than carbon for oxygen: an opinion which they found on this circumſtance, that carbon is precipitated, in Volta's eudiometer, when a mixture of oxygen gas and carbonated hydrogen gas is made to detonate, if a quantity of oxygen ſufficient to acidify the two baſes has not been employed. I ſay, that this affinity was not exerted in my experiment: for it cannot be doubted that the temperature was high enough to reproduce water by the union of the oxygen and hydrogen; and we can here ſee nothing which could decide a preference of the oxygen for the carbon.

Theſe conſiderations ſeemed to me ſufficient to create a new intereſt in regard to the experiment propoſed by C. Clouet. I did not heſitate, therefore, to employ in it one of the diamonds preſerved in the cabinet of the Polytechnic ſchool, according to the leave granted by the Council; being perſuaded that if it diſappeared in the operation, merely by expoſure to a high temperature, in contact with iron, without the acceſſion of the air or any other oxygenating ſubſtance, the fact thereby eſtabliſhed would leave no room to regret having ſacrificed it.

Citizen Clouet had himſelf prepared a ſmall crucible of ſoft iron, forged on purpoſe out of picked heads of nails. Its form was a ſolid of eight planes. (Plate II. fig. 3). It was ſhut by a ſtopper of the ſame iron well adjuſted. (Fig. 4.)

This crucible was to be placed in a Heſſian crucible, furniſhed with a cover well luted. This was all the apparatus for the experiment. I cannot give a better idea of the

reſult than by the report drawn up by C. Clouet, Welter, and Hachette:

Report of the Experiment made at the Polytechnic School, Thermidor 25, Year 7, reſpecting the Converſion of Iron into Steel by the Diamond.

"The diamond employed weighed 907 milligrammes. As it did not entirely occupy the crucible, we filled it with filings of the ſame iron as that of which it was formed. The crucible was ſhut by its iron ſtopper, which was forcibly thruſt home, that as little air as poſſible might remain in the inſide.

	Grammes.
"The crucible and ſtopper weighed together - -	55.8
"The iron filings which covered the diamond -	2
"Total weight of the iron ſurrounding the diamond	57.8

"After having cut off the exceſs of the ſtopper *, the crucible was placed alone, and without the addition of any ſurrounding matter, in a very ſmall Heſſian crucible, and the latter in a ſecond crucible of the ſame earth; but the ſpace between the two latter crucibles was filled with ſiliceous ſand free from all ferruginous particles. In the laſt place, the large crucible was luted with earth ariſing from pounded crucibles and unbaked clay, and the whole was expoſed about an hour to a three-blaſt-forge fire.

"The whole being cooled, we found, in the interior Heſſian crucible, the iron converted into an ingot of caſt ſteel. (See fig. 5.) It formed, with the ſtopper and filings, but one round maſs well terminated, ſome few globules excepted, which were detached, and which weighed only 884 milligrammes.

	Grammes.
"The ingot of caſt ſteel weighed - -	55.500
"The detached globules - - - -	0.884
"Total weight of the ſteel obtained - -	56.384

* This portion of the ſtopper, as well as the remainder of the ingot of which the crucible was formed, were ſubjected to the Claſs for inſpection, in order to certain the nature of the iron employed.

"The

" The iron and the diamond weighed, before the operation, 58.707 grammes; from which it follows that there was a loſs of iron about 2.423 grammes. This iron had given to the Heſſian crucible the colour of plumbago.

" (Signed) CLOUET, WELTER, HACHETTE."

The fuſion of the iron being ſo far perfect as to ſhew on its ſurface the rudiments of the moſt beautiful cryſtalliſation, it is not poſſible to think that any part of the diamond could have remained in the inſide untouched, or that it was not in the moſt intimate ſtate of combination. The difference of the ſpecific gravity oppoſes ſuch an idea *.

Thus the diamond diſappeared by the affinity which iron exerciſed on it by the help of the high temperature to which they were both expoſed, in the ſame manner as a metal diſappears in the allay of another metal.

The diamond, therefore, has furniſhed here the ſame principle as carbon, ſince the product of the union has the ſame properties.

The converſion into ſteel is not doubtful. The ingot having been poliſhed on a lapidary's wheel, a drop of weak nitrous acid immediately produced a dark-grey ſpot, abſolutely like that exhibited on Engliſh caſt ſteel, and on caſt ſteel produced by the proceſs of C. Clouet. Thoſe who have often tried ſteel by this kind of proof, long ago pointed out by Rinmann, had occaſion to remark, that the ſpot of caſt ſteel, though very ſenſible, is however leſs black than that of ſteel made by cementation, which depends perhaps on the different degree of oxydation of the carbon which they have taken in.

Explanation of the FIGURES.

A, fig. 3, (Plate II.) is the plan of the iron crucible. B, a ſection of this crucible. C, fig. 4, the ſtopper of the crucible. D,E, fig. 5, ingot of caſt ſteel ſeen in perſpective. The ſpot formed by the nitric acid on the poliſhed part is repreſented at *a*.

* Some perſons having expreſſed a deſire to ſee the inſide of the ingot, it was broken on the anvil, which was not effected without ſeveral blows from a very large hammer. It divided itſelf into two fragments, which were exhibited at the next ſitting. The fracture appeared perfectly uniform, and of the moſt beautiful grain.

XIX. *Ninth Communication from Dr.* THORNTON, *Phyſician to the General Diſpenſary, relative to Pneumatic Medicine.*

A DISEASED LIVER CURED BY VITAL AIR.

THE butler of Colonel Ironſide, who had been long reſident in India, laboured for ſeveral years under a well-marked liver complaint. He had been under the care of Dr. Warren, and other phyſicians, without experiencing any eſſential advantage. Colonel Ironſide, as all other means had been ineffectual, wiſhed him to try the vital air. He accordingly came under my care, and the ſame tonics were employed as he had before taken; therefore I aſcribe the cure to the vital air, which was conjoined with theſe, and which ſoon completely reſtored him to health; and he has continued well now above a twelvemonth.

Obſervations.

When animals placed in pure vital air were deſtroyed by ſo powerful a ſtimulus, Dr. Beddoes found the liver not liver-coloured, but of a florid red. We therefore can eaſily ſuppoſe it to act on this organ when given in a moderate way: and as the oxyds of mercury and the nitrous acid, which are the beſt remedies for this diſeaſe, act chiefly from their contained oxygen, it is probable that the vital air will hereafter be found a ſpecific in this complaint, poſſeſſing ſuperior advantages over both theſe remedies, and will ſupercede them, although it cannot be put up into two-ounce phials, the principal objection raiſed againſt it; for the airs can now (which originated from my ſuggeſtion) be confined in barrels, and bottled off as eaſily as wine: and I muſt obſerve, that this patient had a barrel of vital air, containing 24 gallons, which coſt him one guinea, conveyed for him to his maſter's ſeat in the country, and a tin pneumatic apparatus for inhaling the medicinal air, which ſtood him at the low rate of thirty ſhillings; which improvements, I truſt, will greatly facilitate the general application of pneumatic medicine, when it will be ſure to find that level its merits entitle it to.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ELECTORAL ACADEMY OF SCIENCES AT MANHEIM.

ON the 16th of April laſt the Electoral Academy of Sciences at Manheim held a public ſitting in commemoration of its deceaſed founder the Elector Charles Theodore, who died on the 17th of February. On this occaſion Mr. M. Collini, a member of the Society, read a printed oration on the viciſſitudes of the Academy. The moſt flouriſhing period of this inſtitution was that between the time of its eſtabliſhment and the year 1777, when the Palatinate family got poſſeſſion of Bavaria, and Charles Theodore transferred his reſidence from Manheim to Munich. It was not, however, left deſtitute of ſupport; but its diſtance from the electoral court rendered many things impoſſible, which, under other circumſtances, might have been accompliſhed. Still more prejudicial to the Academy, as well as the whole Palatinate, was the war which broke out afterwards, between France on the one ſide, and the German Empire and Auſtria on the other. Manheim was bombarded three times, *viz.* in December 1794, November 1795, and January 1798; and as often was it neceſſary to remove the library of the Academy, as well as its papers, &c. to a place of ſafety, and to arrange them. The ſittings and labours of the members were interrupted; the palace, aſſigned to them for holding their meetings, was in part burnt; and as the revenues deſtined for the ſupport of the inſtitution ariſe from poſſeſſions on the left bank of the Rhine, if theſe remain in the hands of the French, it is to be apprehended, that, like many other eſtabliſhments of the ſame kind on the right bank of that river, which had their revenues on the other ſide, it will decline unleſs ſupported by the new regent. Among its moſt diſtin-

guiſhed members, ſince the time of its foundation, may be reckoned Voltaire, elected in 1764; Leſſing, and Lalande the celebrated French aſtronomer. It was eſtabliſhed in 1763 according to a plan of the learned Schöpflin, and divided into two claſſes, the Hiſtorical and Phyſical; the latter of which, in the year 1780, was ſubdivided into the Phyſical (properly ſo called) and Meteorological. The collection of the papers of the Academy have been publiſhed in eleven volumes quarto, under the title of *Acta Academiæ Theodoro-Palatinæ*. The Meteorological Obſervations are from 1781 to 1792, and make twelve volumes quarto, with the title of *Ephemerides Societatis Meteorologicæ Palatinæ*.

SUGAR FROM BEET-ROOTS.

Profeſſor Klaproth has publiſhed the following teſtimony in regard to ſugar procured from beet-roots:

"Having, in conſequence of a requeſt by Mr. Achard, that I would examine his proceſs for making ſugar from beet-roots, and communicate to him the reſult, convinced myſelf, by repeated experiments, of the abundance of ſaccharine matter in theſe roots, that I might aſcertain the proportion with more accuracy, I made the following experiment:—Twenty-five freſh roots, which weighed 32½ pounds after they had been ſcraped and the tops cut off, were put into a perforated tin veſſel, and the juice was expreſſed by a proper apparatus. The juice obtained weighed 19½ pounds. The ſqueezed reſiduum was put into a tin kettle with boiling water, and, after ſtanding an hour, was preſſed alſo. This extract was added to the juice; both were boiled to a ſyrup, and ſtrained through a woollen cloth; after which the ſyrup was put into a porcelain capſule, and evaporated gradually over a ſlow fire to complete dryneſs. The raw ſugar obtained, and which I put into a ſealed glaſs, amounted to two pounds twelve ounces.

"That 25 roots, or 32½ pounds of beets, produced two pounds twelve ounces of ſugar, is hereby certified, as witneſs my hand, MARTIN HENRY KLAPROTH."

"Berlin, 11th January 1799."

A letter

A letter from Berlin, dated September 28, ſtates, that the commiſſion appointed by his Pruſſian majeſty, to examine Achard and Klaproth's proceſs for making ſugar from beet-roots, have finiſhed their report on that ſubject: The reſult is, that 1500 pounds of beet-root gave 398 pounds of very agreeable ſyrup, which produced 57 pounds of powder-ſugar of a white colour, and proper for uſe without being any farther refined.

INDIGO RESIN.

Profeſſor Brugnatelli, of Pavia, has lately announced, that, by treating indigo with the nitrous acid, one may obtain a large quantity of a peculiar reſin, which he calls *Reſina indigofera*. An ounce of indigo diſtilled with four ounces of the nitrous acid, left a thick maſs, in which he found a concrete ſubſtance of a red colour, which, when ſeparated, had all the properties of reſin: it weighed half an ounce. This reſin diſſolved very eaſily in alcohol, which acquired from it a dark red colour. Water decompoſed this ſolution immediately, and produced a yellow precipitate. This tincture communicated its colour to paper and linen, and gave it a beautiful and durable yellow dye. It communicated the ſame colour to the ſkin and the nails: the former retained its colour for a long time. This reſin has a very bitter, aſtringent, and diſagreeable taſte; but no ſenſible ſmell. On glowing coals it melts and burns with a flame, and at the ſame time emits a ſharp diſagreeable ſmell.

CONVERSION OF IRON INTO STEEL.

Our valued correſpondent Mr. Muſhet, of the Clyde Iron-Works, has examined and repeated C. Clouet's proceſs with conſiderable ſucceſs. The experience of Mr. Muſhet in ſimilar proceſſes rendered him well qualified for the inveſtigation, and the reſult has been what we ſhould have promiſed from our knowledge of his abilities and accuracy. Mr. Muſhet, inſtead of experiencing any loſs in the weight of iron employed, has uniformly obtained the ſame increaſe gained in the large way of manufacture by the common proceſs of cementation with charcoal, *viz.* from $\frac{1}{3}$ to $\frac{1}{7}$ th. However, in varying the experiments of the French chemiſts, he has

met with phenomena that force him to draw very different conclusions from them, and to reject the idea of the carbon being furnished by the decompofition of the carbonic acid; for, in fome experiments, he employed for the cementation calcareous earth, previouſly deprived of its acid, and excluded the contact of external air, and the reſult was caſt ſteel. Theſe experiments were performed in crucibles made of Sturbridge clay, without any mixture. Superior effects, however, were produced in half the time, by preſenting carbon, in a comparatively deoxydated ſtate, in crucibles variouſly compounded. Some curious queſtions naturally ariſe from the reſult of theſe experiments; eſpecially from the firſt, as, Whence comes the carbon? But as Mr. Muſhet has promiſed, in ſome future Number, to lay his experiments on this intereſting ſubject before the public, our philoſophical readers will then be better enabled to enter upon the inveſtigation.

IMPROVED WRITING INK.

C. Van Mons has applied the diſcoveries of Prouſt to the preparation of common writing ink. He has found that the ſulphat of iron, calcined to whiteneſs, always gives a moſt beautiful black precipitate. By the following mixture he obtained excellent ink:—Galls, 4 ozs.; ſulphat of iron, calcined to whiteneſs, 2½ ozs.; and two pints of water. The whole muſt be left to infuſe cold for 24 hours; then add gum Arabic 10 drachms, and preſerve it in a ſtone jar, either open, or covered merely with paper.

MANGANESE IN VEGETABLES.

M. Prouſt has lately publiſhed the following ſhort way of proving the preſence of manganeſe in ſuch vegetable productions as contain any; which has this to recommend it, that it is ſhorter and more effectual than that of Scheele. Put in the proceſs diſtilled vinegar (employing heat) on well-waſhed aſhes, without attempting, however, to extract from them all that they are capable of yielding to the acid. The liquor then contains oxyd of manganeſe, lime, and magneſia. Try it by the pruſſiat of pot-aſh, and it gives a precipitate of the colour of peach bloſſoms, which, treated by the blow-pipe in the uſual manner,

manner, gives conſtantly the colour which denotes the preſence of that oxyd. This precipitate is not without a little iron, which it receives, as I believe, from the pruſſiat of pot-aſh. Then put nitric acid over the reſiduum of the aſhes, and it will take from them the iron, which may be proved alſo by the pruſſiat. Then ſearch for the different earths, which may be diſcovered by the common means. The aſhes of the pine, calendula, vine, green oak, and fig-tree, contain manganeſe. The aſhes of the fig-tree are almoſt all ſiliceous; the aſhes of barilla do not contain an atom of it, but iron in great quantity, magneſia, &c.

PREPARATION OF STUFFS FOR DYEING.

A letter from Van Mons to Brugnatelli, printed in the 92d number of the Annales de Chimie, contains a uſeful hint on this ſubject. He expreſſes himſelf as follows:

"The memoir, read by Giobert to the Academy of Turin on the animaliſation of flax and cotton, induces me to mention a circumſtance which ought to have led me to the diſcovery of this animaliſation long ago, had I paid proper attention to it at the time. I had cauſed ſome oyſter-ſhells to be boiled with pot-aſh and lime, in order to whiten them for ſome pharmaceutical purpoſes; and I employed the ley produced by that operation, to bleach the linen cloths uſed to ſtrain the decoctions. My pupils made me afterwards obſerve, that theſe pieces of cloth were ſtrongly dyed when any ſubſtances were paſſed through them, the colour in which might have been carried off by merely waſhing in cold water."

YELLOW DYE FROM A SPECIES OF MUSHROOM.

Among the different kinds of muſhrooms, capable of producing lively and durable dyes, none is more worthy of notice than the *Boletus hirſutus* of Bulliard, from which C. Laſieyrie has extracted a bright, ſhining, and very durable yellow dye. This pretty large muſhroom grows commonly on walnut and apple-trees. Its colouring-matter is contained in abundance, not only in the tubular part, but alſo in the parenchyma of the body of the muſhroom. In order

to extract it the muſhroom is pounded in a mortar, and the liquor thence obtained is boiled for a quarter of an hour in water. An ounce of liquor is ſufficient to communicate colouring-matter to ſix pounds of water. When the liquor has been ſtrained, the ſtuff to be dyed is put into it, and boiled for a quarter of an hour. All kinds of ſtuff receive this colour and retain it, but on linen and cotton it is leſs bright. This colour may be modified, in a very agreeable manner, by the effect of mordants.

The proceſs ſucceeded beſt on ſilk. When this ſubſtance, after being dyed, is made to paſs through a bath of ſoft ſoap, it acquires a ſhining golden-yellow colour, which has a perfect reſemblance to the yellow of that ſilk employed to imitate embroidery in gold, and which has hitherto been brought from China and ſold at a dear rate, as the method of dyeing it is unknown in Europe. The yellow colour extracted from this muſhroom may be employed alſo with advantage for painting in water-colours as well as in oil.

NATURAL HISTORY.

The two following articles on the Friendſhip and the Sagacity of Birds, by Mr. Simpſon of New-York, we copy from the Medical Repoſitory, a valuable work publiſhed in America:—

"Mr. Myers, a brother-in-law of mine, moving from Wilton to Philadelphia, deſired me to ſend for a large turkey-cock and hen, and a pair of bantams, which had been a long time in his yard, and which the family did not chooſe to have killed. Accordingly, after his departure, I had them brought home, and put with ſome other poultry that were then running in my yard. Some time after, as I was feeding the poultry from the barn-door, a large hawk turned the barn, and ſuddenly made a pitch at the bantam hen: ſhe immediately gave the alarm, by a noiſe which they generally make on ſuch occaſions; when the large turkey-cock, who was about two yards diſtance, and who I preſumed ſaw the hawk's intentions, and the imminent danger of his old acquaintance, flew at the hawk with ſuch violence, and gave him ſuch a ſevere ſtroke with his ſpurs as he was going to

ſeize

ſeize his prey, as to knock him from the hen to a conſiderable diſtance; and the timely aid of this faithful auxiliary, the turkey-cock, ſaved the bantam from being devoured by the hawk."

During my reſidence at Wilton, early one morning I heard a noiſe from a couple of martins, who were jumping from tree to tree adjoining my dwelling. They made ſeveral attempts to get in a box or cage fixed againſt the houſe, which they had before occupied; but they always appeared to fly from it with the greateſt dread, and repeated thoſe loud cries which firſt drew my attention. Curioſity led me to watch their motions. After ſome time a ſmall wren came from the box or cage, and perched on a tree near it, when her ſhrill notes ſeemed to amaze her antagoniſts. After ſome time ſhe flew away. The martins took this opportunity of returning to their cage: but their ſtay was ſhort. Their diminutive adverſary returned, and made them fly with the greateſt precipitation. They continued manœuvring in this way the whole day, and I believe the wren kept poſſeſſion during the night. The following morning, on the wren's quitting the cage, the martins immediately returned, took poſſeſſion of their manſion, broke up their own neſt, which conſiſted of twigs of different ſizes; went to work, and with more induſtry and ingenuity than I ſuppoſed they poſſeſſed, they ſoon barricaded their doors. The wren returned, but could not re-enter. She made attempts to ſtorm the works, but did not ſucceed.—I will not preſume to ſay that the martins followed our modern maxim, and carried with them a ſufficiency of food to ſuſtain a ſiege, or that they made uſe of the abſtinence which neceſſity ſometimes, during a long and bad ſtorm, might probably occaſion; but they perſevered for near two days, to defend the entrance within the barricado: and the wren, finding ſhe could not force an entry, raiſed the ſiege, quitted her intentions, and left the martins in quiet poſſeſſion without further moleſtation.

DEATHS.

On Friday, the 11th of October, Samuel More, Eſq. who for many years paſt filled the office of Secretary to the Society

for the Encouragement of Arts, Manufactures, and Commerce. The aſſiduity and ability with which he diſcharged the duties of the office he held, for that reſpectable Inſtitution, will make it difficult to find another to fill it who may be as well qualified.

A few weeks ago, at New-York, Dr. Perkins, inventor of the metallic tractors. He had gone thither, from his uſual place of reſidence in Connecticut, to aſſiſt his brother practitioners in endeavouring to arreſt the progreſs of the yellow fever, with which that city has been again viſited, and caught the infection, which carried him off.

In the Philoſophical Magazine for April laſt we announced the death of that celebrated naturaliſt Spallanzani on the 11th of February. It appears, however, that he died on the 17th; and, as every thing that relates to the laſt moments of eminent men is intereſting, we hope our readers will not be diſpleaſed with us for adding the following particulars:—On the evening of the 10th he was ſeized with an apoplectic fit, the conſequence of a neglected ſtrangury. Profeſſors Brera and Scarpa, who were called in to viſit him, immediately ſaw that all medical aid would be uſeleſs; for, on inſpecting his urine, it was found that the bladder was in a ſtate of gangrene. They however ordered him ſuch medicines and treatment as they thought could be of any ſervice. On the 12th the affection in the left arm and the lips diſappeared. On the 15th he recovered the full uſe of his ſenſes; found himſelf much better; and, without apprehending death, attended to his literary and domeſtic concerns. Early on the morning of the 17th he was attacked by a general debility, accompanied with a diarrhœa entirely colliquative. The two profeſſors then told him, that this day would be the laſt of his life; an information which Spallanzani received with the greateſt firmneſs of mind. Towards noon he embraced his friends, the two profeſſors, and particularly his brother, who had come expreſs to Pavia from Scandiano, in the duchy of Modena; took the moſt affectionate leave of them, and exactly at 11 o clock breathed his laſt in their arms. The opinion of the phyſicians reſpecting his diſeaſe was confirmed by the opening of his body. A hiſtory of it has been publiſhed by Profeſſor

Profeſſor Brera, who was two years phyſician to this eminent naturaliſt. Two monuments are to be erected at the public expence to his memory; one in the Pantheon of Milan, and another within the precincts of the Univerſity of Pavia.

In the ſame Number of the Philoſophical Magazine we announced alſo the death of Profeſſor Lichtenberg, of Gottingen, on the 24th of February. Since that period a ſmall work has appeared at Gottingen under the title of, *Elogium Georgii Chriſtophori Lichtenberg, in conceſſu Soc. Reg. Scientiarum recitavit A. G. Kaſtner, 20mo. d. Ap.* 1799; from which we extract the following particulars:—Profeſſor Lichtenberg was born on the 1ſt of July 1744, at Oberamſtadt, in Heſſe Darmſtadt. He was of a weakly conſtitution from his birth, and applied himſelf very early to the mathematics and natural hiſtory, in which he was greatly aſſiſted by his two brothers, who were older. In the year 1763 he went to Gottingen, where he became the pupil and friend of Kaſtner and Meiſter, and attended the Lectures of the other profeſſors. In conjunction with Erxleben he made ſome obſervations on the earthquake of 1767; and remarked, in particular, that the ſhock continued ſix ſeconds; while others, not accuſtomed to mathematical accuracy, extended its duration to a whole minute. He delineated the courſes of the comets which had been obſerved; and it is remarked, that he poſſeſſed a great deal of patience for works of this kind. Profeſſor Kaſtner has now in his poſſeſſion a drawing by him, of the face of the moon, where the ſpots are marked in the order in which they enter the earth's ſhadow during eclipſes. In the year 1770 he was invited to Gieſſen; but he preferred Gottingen, where he was appointed Profeſſor Extraordinary. In the years 1772 and 1773 he was commiſſioned to determine, from aſtronomical obſervations, the poſition of various places in his Britannic Majeſty's German States; and for this purpoſe was ſupplied, at the king's expence, with a quadrant by Siſſon. In 1776 he laid the reſult of this labour before the Royal Society, of which he had been elected a member two years before. As repeated wiſhes had been expreſſed that Mayer's manuſcripts might be publiſhed, Profeſſor Kaſtner put them into the hands of Lichtenberg, who, aſſiſted by Dietrich, publiſhed

publiſhed the firſt volume, in 1775, with a typographic elegance which had been before unknown in Germany. Want of leiſure prevented him from editing a ſecond. Lichtenberg was twice in England; the firſt time in 1770, and again in 1774. In the year 1777, after Erxleben's death, he ſtudied natural philoſophy, in the introduction to that ſcience written by his predeceſſor; the four laſt editions of which were publiſhed under his inſpection, but always with additions by himſelf. On this occaſion he made a collection of philoſophical inſtruments, which afterwards became the property of the Univerſity. Among the new experiments, which were peculiarly Lichtenberg's own, is that reſpecting the figures formed by pounded reſin on the electrophore, by which he examined and traced out the nature and progreſs of the electric fluid in a new manner, and communicated the reſult of his reſearches in two papers, read before the Royal Society in 1777 and 1778. Profeſſor Lichtenberg had a very extenſive correſpondence, which enabled him to communicate to the Society a variety of uſeful intelligence. He propoſed, alternately with Kaſtner, the prize queſtions of the mathematical claſs, and was the author of the laſt for the preſent year reſpecting the motion of ſteam in conductors of a given kind. In the year 1780 he began, in conjunction with the late J. R. Forſter, the Gottingen Magazine, ſix numbers of which appeared annually, till 1785, when it was dropped with the firſt number of that year. In the Gottingen Pocket Calendar, of which he was the editor for many years, he publiſhed various articles equally uſeful and agreeable. Among theſe was an account of Hogarth's ſatyrical prints, accompanied with engravings. The large account of theſe prints, with a commentary, is well known. As Profeſſor Lichtenberg's complaints returned regularly every year, the laſt attack was at firſt not conſidered dangerous; but an inflammation of the lungs taking place, he was carried off on the 24th of February, as before ſtated.

THE

PHILOSOPHICAL MAGAZINE.

NOVEMBER 1799.

I. *Observations on the Flux and Reflux of the Atmosphere.* By the *Abbé* MANN*.

AS the general power of attraction or gravity extends to all bodies within the sphere of its action; and as the atmosphere of our earth consists not only of heavy, but also of moveable and elastic parts, which revolve around a common axis like the water of the ocean, it necessarily follows that the atmosphere must be affected by the same physical causes which produce the flux and reflux of the sea. This consequence is so immediate and necessary that it is not possible to doubt it: the question therefore will not be the existence of a flux and reflux, but merely respecting their extent. Many philosophers have asserted, on grounds which do not appear to have much validity, that the flux and reflux of the atmosphere amount to no more than those of the ocean, that is, eight feet. According to their opinion, a sea of water, air, or quicksilver, would be raised to about the same height by the effects of the sun and moon, and acquire from them nearly the same movement. Those who speak in this manner, must entirely overlook the nature of elastic fluids; for it may easily be conceived that, with the same power of at-

* From the *Transactions of the Academy of Sciences at Brussels*, Vol. IV.

traction, the movement of an elaſtic fluid would be totally different from that of a non-elaſtic. Other philoſophers, not neglecting this conſideration, have believed that the cauſes which produce the movement of the waters of the ocean can occaſion a much greater in the aërial maſs of the atmoſphere, and muſt have a very great ſhare in producing the regular winds, and other phenomena which depend on them.

D'Alembert has calculated, from the theory of general gravitation, thoſe movements which muſt be produced in the atmoſphere by the effects of the ſun and moon. He found that from this influence a continual eaſt-wind muſt ariſe at the equator, and that, in the temperate zones, not far from the tropics, it muſt be converted into a weſt-wind; that on account, however, of many local circumſtances, and other impediments which come in the way, it cannot always continue in the ſame direction; and that the variations in the ſtate of the barometer, which thence ariſe, muſt be very inconſiderable, and almoſt imperceptible.

Bacon, Gaſſendi, Deſchales, Goad, Dampier, Halley, and others who have written on the wind, all unanimouſly obſerve, that the periods of the year moſt expoſed to it are the two equinoxes: that ſtorms are moſt frequent at the times of new and full moon, and particularly thoſe which happen near the equinoxes: that at periods, otherwiſe calm, a ſmall breeze always takes place at high water; and that a ſmall movement in the atmoſphere is each time perceived a little after noon and midnight. Now, as moſt of theſe circumſtances have a great reſemblance to thoſe which accompany the flux and reflux of the ſea, and take place at the ſame time; and as in this reſpect the movement of water and the atmoſphere is the ſame, according to Newton's rule, that natural effects of the like kind muſt be produced by the ſame cauſes *, both may be deduced from the ſame ſource.

From this it appears that theory and experience coincide to eſtabliſh the exiſtence of an atmoſpheric flux and reflux, and of the regular and very perceptible effects which they produce. Nothing therefore is neceſſary but to deduce their

* Effectuum naturalium ejuſdem generis eædem aſſignandæ ſunt cauſſæ.

phenomena

phenomena and extent from the well-known properties of elaſtic fluids. For this purpoſe we muſt have before our eyes the following principles of aerometry, to be found in moſt elementary books:—

1. The elaſticity of fluids is in the inverſe ratio of their denſity, and in the direct ratio of their rarity. Thus air is more elaſtic than water; light more elaſtic than air; and ether than light. This is one of the principles of Newton *.

2. The force or elaſticity of the air expands and contracts itſelf in the direct ratio of the weight with which it is loaded, and diffuſes itſelf in the inverſe ratio of the force by which it is compreſſed †.

3. The air is rarefied, or diffuſes itſelf, in the direct ratio of the quantity of heat which acts upon it ‡.

4. The air, as well as all fluids in general, has a tendency to put itſelf in equilibrium, and does not reſt until it has obtained it §.

It now follows, from theſe principles, that every thing which increaſes the weight of the atmoſphere, and preſſes the air more in one place than another near it, muſt occaſion the air to ruſh from the former to the latter, where it is lighter: and, on the other hand, every thing that leſſens the gravity of the atmoſphere, and increaſes or rareſies the air, and makes it lighter than in the neighbouring parts, muſt occaſion the air to ruſh in from all ſides to thoſe parts: and this continues in each caſe till the equilibrium is reſtored, and reſt again effected. This is the principal cauſe of the origin of wind. But, as a pendulum put in motion does not immediately reſt when it has come to a perpendicular direction, but obtains reſt after making ſome ſmall vibrations; in the ſame manner, the currents of air, from the places where the air is more preſſed to that where it is leſs ſo, will ruſh beyond the boundaries of the equilibrium, from which they will again fly back, and at length obtain reſt after ſeveral undulations. As this takes place in all fluids, it is more

* Princip. in fine, p. 530, edit. 1726, et quæſt. 21, 22. in fine optices.
† Wolf Elem. Aërom. § 72—77. ‡ Ibid. § 146. § Ibid. § 36—44.

peculiarly common to elaſtic fluids; and it is obſerved, according to theſe principles, that the winds during ſtorms change their courſe, and blow exactly from thoſe points to which the current was before directed.

The ſpecific gravity of the air is 800 times leſs than that of water, and its elaſticity is infinitely greater. The attractive power of the ſun and moon leſſens the gravity and preſſure of the atmoſphere towards the earth, as it draws it towards theſe bodies in the ſame manner as it does the water of the ocean. But this power of attraction does not extend the water of the ocean, which is deſtitute of elaſticity, as it does the whole maſs of the atmoſphere (poſſeſſed of it, and expoſed to the action of this power) in the ratio of the decreaſe of gravity, and of the preſſure of the atmoſphere towards the earth. Adopting, therefore, the inadmiſſible ſuppoſition, that water, air, and quickſilver would be raiſed to nearly the ſame height, and be moved in the ſame manner by the attractive force of the ſun and moon; this much at any rate is proved, that the extenſion of the air, in proportion to the leſſening of the gravity of the atmoſphere towards the earth, muſt always take place, and produce a conſiderable atmoſpheric flux, while this effect cannot occur in regard to the flux of the ocean. It is certain that fluids are more or leſs ſubject to the effects of any power acting upon them, according as they have more or leſs mobility, which is in the direct ratio of their rarity and elaſticity. Now, as air poſſeſſes theſe properties in a degree 800 times greater than water, the flux of the atmoſphere muſt very perceptibly exceed in magnitude that of water. This, therefore, is a ſecond certain argument againſt thoſe who pretend that the flux and reflux of the atmoſphere are ſimilar to thoſe of the ocean. Not only muſt the attractive power of the ſun and moon, on account of the greater elaſticity and mobility of the air compared with water, produce a greater effect on the atmoſphere than on the ocean, but the component parts of the atmoſphere, as they are nearer the moon by about a ninetieth part of the ſemidiameter of the earth, muſt be more ſtrongly attracted than the watery particles of the ocean; in the ſame manner as thoſe which are 90 degrees

diſtant

diſtant from the place where the moon is vertical, muſt occaſion a greater preſſure on the earth on account of the greater obliquity of the attraction, and this in proportion to the greater height of the atmoſphere above that of the ſea. This is the third phyſical cauſe which makes the flux and reflux of the atmoſphere more conſiderable than that of the ocean. To this we may add the immenſe ſpace occupied by the atmoſphere in compariſon of the ocean; for if the whole globe were covered with water, this general ocean would not occupy the fiftieth part of the ſpace actually filled by the atmoſphere; and in my opinion the extenſion effected in elaſtic fluids, by the ſame power of attraction, is in proportion to their maſſes. Suppoſing this to be the caſe, the flux of air in an atmoſphere of ten miles height, would be much more conſiderable than in another amounting to only a tenth part of that height: though this difference cannot take place in the flux and reflux of the ocean, becauſe water is deſtitute of elaſticity. Beſides, lands, iſlands, ſtraits, bays, the ſituation of the coaſts, ſand-banks, ſhoals, &c. throw a great many impediments in the way of the water in regard to its motion; whereas the atmoſphere, which riſes to a height ten or twelve times greater than the ſummits of the higheſt mountains, is in no manner prevented from moving according to the effects of the power of attraction, unleſs ſomething very particular takes place in its loweſt ſtrata. It is more than probable that each of theſe cauſes contributes its part to make the movement of the atmoſphere far more conſiderable than that of the ocean.

It is therefore certain that the united effects of the attraction of the ſun and moon on the atmoſphere of the earth muſt raiſe and extend it in the inverſe ratio of the ſquare of the diſtance ſo as to make it aſſume the form of a lengthened ſpheroid, the greater diameter of which, from the ſame grounds and with the ſame variations as the aqueous ſpheroid, muſt nearly follow the direction of the line of attraction.

Beſides, the heat of the ſun, which has no ſenſible influence in raiſing or extending the water of the ocean, will produce an effect on that part of the atmoſphere expoſed to its

rays

rays by heating and rarefying it, in the ratio of its ſtrength, according to the third of the before-mentioned principles. That part, therefore, of the atmoſphere which is gradually turned towards the ſun, muſt extend in proportion to the degree of heat which acts upon it, and riſe above the reſt; and this atmoſpheric ſwelling will conſtantly follow the apparent daily courſe of the ſun. The moon, on the other hand, has no power to produce or change this phenomenon; becauſe, in all the experiments hitherto made to collect and concentrate its rays, the ſmalleſt degree of heat has never been perceived.

As there are therefore two different cauſes, *viz.* the united attraction of the ſun and moon, and the heat of the ſun alone; by which the latter, except at the time of the ſyzegies, when they both act in the ſame direction upon one point, has a particular influence on the atmoſphere of the earth independent on that of the former, it thence follows that they will produce three different fluxes every day. Two of them ariſe from the attractive power of the ſun and moon, and in their formation, direction, and movement, are perfectly ſimilar to thoſe produced in the ocean from the ſame cauſes. The third, however, is produced by the heat of the ſun alone; and its prominent parts will always be in that parallel through which the ſun paſſes in the courſe of his daily movement, and will continually follow that luminary, from parallel to parallel, at a ſmall diſtance. The two firſt I ſhall call *attraction-tides*, the third *heat-tides*.

The tides of attraction, like thoſe of the ocean, and from the like grounds, have at the ſame time, at two oppoſite ends of the globe, projecting parts, and theſe lie almoſt in that line which might be drawn from the centre of the earth to that of the moon. The heat-tides, on the other hand, can take place only on one point of the globe; that is, in the point to which the ſun is vertical. Their projecting part will be directed towards that luminary, and nearly follows its movement.

In regard to the effects of theſe atmoſpheric tides, they depend on the natural ſtate of the aërial fluid, and on its reſt, which conſiſts in an equilibrium of all its parts. When this

this equilibrium is deſtroyed, the atmoſphere returns to its former ſtate as ſoon as the cauſe of this derangement is removed. If this equilibrium is by any cauſe deſtroyed in any one place, a movement muſt naturally follow from all ſides, and continue until the equilibrium is again reſtored; and in this we may diſcover the principal cauſe of the origin of wind. Every thing, therefore, which can derange the equilibrium of the atmoſphere in any manner, muſt be reckoned among thoſe cauſes which give riſe to wind: ſuch as the flux and reflux of the atmoſphere, occaſioned by attraction and heat; all other rarefaction of the air by heat; or the condenſation of it by cold, &c.; the letting looſe of vapours by fermentation and evaporation into the atmoſphere; the eruption from the boſom of the earth and aſcent of elaſtic vapours, volcanoes, earthquakes, and perhaps other cauſes not ſo evident, and therefore leſs known. The exceedingly variable directions of the wind depend on the nature of the countries from which they blow; and, in particular, on mountains, foreſts, rivers, marſhes, lakes. In a word, every thing that can oppoſe or favour the free movement of the aërial fluid has an influence on the direction of the wind.

Among all thoſe cauſes which derange the equilibrium of the atmoſphere, and contribute to the production of wind, the principal and moſt uniform is the rarefaction and condenſation of the air. Both are the immediate effect of the different atmoſpheric tides. The regular courſe of theſe tides from eaſt to weſt, in conſequence of that of the ſun and moon, muſt produce a continual eaſt wind; which is however exceedingly weak, but continues without interruption, and prevails only in the immenſe ſeas of the Torrid zone, though with moſt regularity in the Pacific ocean, on account of its vaſt extent. The compreſſed parts of the atmoſphere in the neighbourhood ruſh towards thoſe moſt rarefied by the atmoſpheric tides, and follow them alſo in their regular progreſs from eaſt to weſt. The parts of the atmoſphere, however, in the weſt, will have a much leſs, or perhaps no movement towards thoſe which proceed from the eaſt, on account of the atmoſpheric flux moving towards them: at any rate, the motion from eaſt to weſt will exceed that ſmall motion

from weſt to eaſt, and therefore produce a continual eaſt wind in the parallel of the atmoſpheric tide. A wind alſo will proceed from the north or ſouth of this parallel, in an oblique direction north and ſouth, to the diſtance of about thirty degrees on each ſide of the equator. In both the temperate zones the regular winds muſt blow from weſt to eaſt within theſe boundaries, as they incline towards the parallel of the atmoſpheric tide, and again reſtore the equilibrium of the atmoſphere, which has been rareſied by the continual movement of the aërial tides. Theſe conſequences, deduced from theory, coincide perfectly with the courſe of the wind in the ocean. At land, and on confined ſeas, many other cauſes exiſt which contribute to change the regular direction of the winds, and to make them variable.

On the coaſts, in the torrid zone, the wind blows for the moſt part from the ſea towards the land. The reaſon of this is evident. The reflected rays of the ſun, and other cauſes, heat and rarefy the air at land much more than at ſea; and the direction of the wind always proceeds towards the rareſied part of the atmoſphere. The places where the laſt mentioned winds ſeparate themſelves from the regular winds, muſt be abſolutely calm. All this is confirmed by experience throughout the whole torrid zone, and particularly on the coaſt of Guinea.

Beſides the determination of the courſe of the winds in conſequence of the atmoſpheric flux and reflux, there are an immenſe number of local and accidental cauſes which have an influence on the creation and direction of the winds. One of the leaſt irregular is the preſſure and gravity of the atmoſphere in the frigid zones, which muſt occaſion a continued movement of the air towards the more rareſied parts, and at the ſame time a wind from the poles towards the equator. As ſoon, however, as this continual wind enters the temperate zones, its direction will be overcome and deranged by other local cauſes: an examination, however, of thoſe winds not ariſing from the atmoſpheric tides, does not fall within the plan of this eſſay.

Thoſe winds, on the other hand, which generally blow at ſun-riſe and ſun-ſet, are a remarkable effect of the atmoſpheric

spheric tides produced by heat, as the gentle wind observed at the times of high water in the ocean, even during weather in other respects calm, proceeds from an atmospheric tide produced by attraction.

The difficulty of breathing which people experience in the torrid zone, under an atmospheric tide, arising from heat, must be ascribed as much to a rarefaction of the air thereby occasioned, as to the heat itself. A like effect, in regard to breathing, is perceived on high mountains; which however arises from the thinness of the air on these eminences, for here the heat can have no part in the rarefaction.

It happens sometimes, in warm countries, that during the atmospheric tides, the heat and rarefaction of the air increase to such a degree that they create scorching and suffocating winds, known under the name of *Solanos*; the violence of which proceeds so far sometimes, that they destroy on the spot those who are immediately exposed to them. Abundant and horrid instances of this circumstance are quoted by the Abbé Richard in his Natural History of the Air and Meteors which have occurred in the Deserts of Arabia, in the neighbourhood of the Persian Gulph, and other places.

Besides these *solanos*, it is observed also, sometimes, that the wind suddenly rushes from all quarters to that point where the air is most rarefied, which occasions storms and hurricanes in that part by the mutual shock of the air and the vapours streaming in all directions towards one centre, and the winds then blow backwards again from this point to every quarter of the heavens till the equilibrium is restored. This effect, so natural and common, has long astonished those who never employed themselves in endeavouring to discover the cause.

In the last place, there is good reason for conjecturing that the same physical causes which produce the different atmospheric tides, with their consequences, contribute also to change the weather and temperature, and to produce at the same time a great many other meteorological phenomena.

II. *On the Nature and Conſtruction of the Sun and Fixed Stars.* *By* WILLIAM HERSCHEL, *LL.D. F.R.S.**

AMONG the celeſtial bodies, the Sun is certainly the firſt which ſhould attract our notice. It is a fountain of light that illuminates the world! It is the cauſe of that heat which maintains the productive power of Nature, and makes the earth a fit habitation for man! It is the central body of the planetary ſyſtem; and what renders a knowledge of it ſtill more intereſting to us is, that the numberleſs ſtars which compoſe the univerſe, appear, by the ſtricteſt analogy, to be ſimilar bodies. Their innate light is ſo intenſe, that it reaches the eye of the obſerver from the remoteſt regions of ſpace, and forcibly claims his notice.

Now, if we are convinced that an enquiry into the nature and properties of the ſun is highly worthy of our notice, we may alſo, with great ſatisfaction, reflect on the conſiderable progreſs that has already been made in our knowledge of this eminent body. It would require a long detail to enumerate all the various diſcoveries which have been made on this ſubject; I ſhall therefore content myſelf with giving only the moſt capital of them.

Sir Iſaac Newton has ſhewn that the ſun, by its attractive power, retains the planets of our ſyſtem in their orbits: he has alſo pointed out the method whereby the quantities of matter which it contains may be accurately determined. Dr. Bradley has aſſigned the velocity of the ſolar light with a degree of preciſion exceeding our utmoſt expectation. Galileo, Scheiner, Hevelius, Caſſini and others have aſcertained the rotation of the ſun upon its axis, and determined the poſition of its equator. By means of the tranſit of Venus over the diſk of the ſun, our mathematicians have calculated its diſtance from the earth; its real diameter and magnitude; the denſity of the matter of which it is compoſed; and the fall of heavy bodies on its ſurface.

* From the *Philoſophical Tranſactions of the Royal Society for* 1794, Part I.

From

From the particulars here enumerated it is ſufficiently obvious, that we have already a very clear idea of the vaſt importance and powerful influence of the ſun on its planetary ſyſtem: and if we add to this the beneficent effects we feel on this globe from the diffuſion of the ſolar rays; and conſider that, by well-traced analogies, the ſame effects have been proved to take place on other planets of this ſyſtem, I ſhould not wonder if we were induced to think that nothing remained to be added in order to complete our knowledge: and yet it will not be difficult to ſhew that we are ſtill very ignorant, at leaſt with regard to the internal conſtruction of the ſun. The various conjectures which have been formed on this ſubject, are evident marks of the uncertainty under which we have hitherto laboured.

The dark ſpots in the ſun, for inſtance, have been ſuppoſed to be ſolid bodies revolving very near its ſurface. They have been conjectured to be the ſmoke of volcanoes, or the ſcum floating upon an ocean of fluid matter. They have alſo been taken for clouds. They were explained to be opaque maſſes ſwimming on the fluid matter of the ſun, dipping down occaſionally. It has been ſuppoſed that a fiery liquid ſurrounded the ſun, and that, by its ebbing and flowing, the higheſt parts of it were occaſionally uncovered, and appeared under the ſhape of dark ſpots; and that, by the return of the fiery liquid, they were again covered, and in that manner ſucceſſively aſſumed different phaſes. The ſun itſelf has been called a globe of fire, though perhaps metaphorically. The waſte it would undergo by a gradual conſumption, on the ſuppoſition of its being ignited, has been ingeniouſly calculated: and, in the ſame point of view, its immenſe power of heating the bodies of ſuch comets as draw very near to it has been aſſigned.

The bright ſpots, or faculæ, have been called clouds of light, and luminous vapours. The light of the ſun itſelf has been ſuppoſed to be directly inviſible, and not to be perceived unleſs by reflection; though the proofs which are brought in ſupport of that opinion ſeem to me to amount to no more than, what is ſufficiently evident, that we cannot ſee when rays of light do not enter the eye.

But it is time to profit by the many valuable obſervations we are now in poſſeſſion of. A liſt of ſucceſſive eminent aſtronomers may be named, from Gallileo down to the preſent time, who have furniſhed us with materials for examination.

In ſupporting the ideas I ſhall propoſe in this paper, with regard to the phyſical conſtruction of the ſun, I have availed myſelf of the labours of all theſe aſtronomers, but have been induced thereto only by my own actual obſervation of the ſolar phenomena; which, beſides verifying thoſe particulars that had been already obſerved, gave me ſuch views of the ſolar regions as led to the foundation of a very rational ſyſtem. For, having the advantage of former obſervations, my lateſt reviews of the body of the ſun were immediately directed to the moſt eſſential points; and the work was by this means facilitated, and contracted into a pretty narrow compaſs.

The following is a ſhort extract of my obſervations on the ſun, to which I have joined the conſequences I now believe myſelf entitled to draw from them. When all the reaſonings on the ſeveral phenomena are put together, and a few additional arguments taken from analogy, which I ſhall alſo add, are properly conſidered, it will be found that a general concluſion may be made which ſeems to throw a conſiderable light on our preſent ſubject.

In the year 1779 there was a ſpot on the ſun which was large enough to be ſeen with the naked eye. By a view of it with a ſeven feet reflector, charged with a very high power, it appeared to be divided into two parts. The largeſt of the two, on the 19th of April, meaſured 1′8″,06 in diameter; which is equal in length to more than 31 thouſand miles. Both together muſt certainly have extended above 50 thouſand.

The idea of its being occaſioned by a volcanic exploſion violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus reſtore the ſun, in that place, to its former ſplendour, ought to be rejected on many accounts. To mention only one, the great extent of the ſpot is very unfavourable to ſuch a ſuppoſition. Indeed a much

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leſs violent and leſs pernicious cauſe may be aſſigned to account for all the appearances of the ſpot. When we ſee a dark belt near the equator of the planet Jupiter, we do not recur to earthquakes and volcanoes for its origin. An atmoſphere, with its natural changes, will explain ſuch belts. Our ſpot on the ſun may be accounted for on the ſame principles. The earth is ſurrounded by an atmoſphere compoſed of various elaſtic fluids. The ſun alſo has its atmoſphere; and if ſome of the fluids which enter into its compoſition ſhould be of a ſhining brilliancy, in the manner that will be explained hereafter, while others are merely tranſparent, any temporary cauſe which may remove the lucid fluid will permit us to ſee the body of the ſun through the tranſparent ones. If an obſerver were placed on the moon, he would ſee the ſolid body of the earth only in thoſe places where the tranſparent fluids of our atmoſphere would permit him. In others, the opaque vapours would reflect the light of the ſun without permitting his view to penetrate to the ſurface of our globe. He would probably alſo find that our planet had occaſionally ſome ſhining fluids in its atmoſphere; as, not unlikely, ſome of our northern lights might not eſcape his notice, if they happened in the unenlightened part of the earth, and were ſeen by him in his long dark night. Nay, we have pretty good reaſon to believe, that probably all the planets emit light in ſome degree; for the illumination which remains on the moon in a total eclipſe cannot be entirely aſcribed to the light which may reach it by the refraction of the earth's atmoſphere. For inſtance, in the eclipſe of the moon October 22, 1790, the rays of the ſun refracted by the atmoſphere of the earth towards the moon, admitting the mean horizontal refraction to be $30' 50'',8$, would meet in a focus 189 thouſand miles beyond the moon; ſo that conſequently there could be no illumination from rays refracted by our atmoſphere. It is, however, not improbable, that about the polar regions of the earth there may be refraction enough to bring ſome of the ſolar rays to a ſhorter focus. The diſtance of the moon at the time of the eclipſe would require a refraction of $54' 6''$, equal to its horizontal parallax

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at that time, to bring them to a focus ſo as to throw light on the moon.

The unenlightened part of the planet Venus has alſo been ſeen by different perſons, and not having a ſatellite, thoſe regions that are turned from the ſun cannot poſſibly ſhine by a borrowed light; ſo that this faint illumination muſt denote ſome phoſphoric quality of the atmoſphere of Venus.

In the inſtance of our large ſpot on the ſun, I conclude from appearances that I viewed the real body of the ſun itſelf, of which we rarely ſee more than its ſhining atmoſphere.

In the year 1783 I obſerved a fine large ſpot, and followed it up to the edge of the ſun's limb. Here I took notice that the ſpot was plainly depreſſed below the ſurface of the ſun; and that it had very broad ſhelving ſides. I alſo ſuſpected ſome part, at leaſt, of the ſhelving ſides to be elevated above the ſurface of the ſun; and obſerved that, contrary to what uſually happens, the margin of that ſide of the ſpot which was fartheſt from the limb was the broadeſt.

The luminous ſhelving ſide of a ſpot may be explained by a gentle and gradual removal of the ſhining fluid, which permits us to ſee the globe of the ſun. As to the uncommon appearance of the broadeſt margin being on that ſide of the ſpot which was fartheſt from the limb when the ſpot came near the edge of it, we may furniſh that the ſun has inequalities on its ſurface, which may poſſibly be the cauſe of it. For, when mountainous countries are expoſed, if it ſhould chance that the higheſt part of the landſcape are ſituated ſo as to be near that ſide of the margin, or penumbra of the ſpot, which is towards the limb, it may partly intercept our view of it when the ſpot is ſeen very obliquely. This would require elevations at leaſt five or ſix hundred miles high; but conſidering the great attraction exerted by the ſun upon bodies at its ſurface, and the ſlow revolution it has upon its axis, we may readily admit inequalities to that amount. From the centrifugal force at the ſun's equator, and the weight of bodies at its ſurface, I compute that the power of throwing down a mountain by the exertion of the former, balanced by the

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superior force of keeping it in its place of the latter, is near 6½ times less on the sun than on our equatorial regions; and as an elevation similar to one of three miles on the earth would not be less than 334 miles on the sun, there can be no doubt but that a mountain much higher would stand very firmly. The little density of the solar body seems also to be in favour of the height of its mountains; for, *cæteris paribus*, dense bodies will sooner come to their level than rare ones. The difference in the vanishing of the shelving side, instead of explaining it by mountains, may also, and perhaps more satisfactorily, be accounted for from the real difference of the extent, the arrangement, the height, and the intensity of the shining fluid, added to the occasional changes that may happen in these particulars during the time in which the spot approaches to the edge of the disk. However, by admitting large mountains on the face of the sun, we shall account for the different opinions of two eminent astronomers; one of whom believed the spots depressed below the sun, while the other believed them elevated above it. For it is not impossible that some of the solar mountains may be high enough occasionally to project above the shining elastic fluid, when, by some agitation, or other cause, it is not of the usual height: and this opinion is much strengthened by the return of some remarkable spots which served Cassini to ascertain the period of the sun's rotation. A very high country, or chain of mountains, may oftener become visible, by the removal of the obstructing fluid, than the lower regions, on account of its not being so deeply covered with it.

In the year 1791 I examined a large spot on the sun, and found it evidently depressed below the level of the surface; about the dark part was a broad margin, or plane, of considerable extent, less bright than the sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.

In confirmation of these appearances, I carefully remarked that the disk of the sun was visibly convex: and the reason of my attention to this particular, was my being already long acquainted with a certain optical deception, that takes place now and then when we view the moon; which is, that all the

the elevated ſpots on its ſurface will ſeem to be cavities, and all cavities will aſſume the ſhape of mountains. But then, at the ſame time, the moon, inſtead of having the convex appearance of a globe, will ſeem to be a large concave portion of an hollow ſphere. As ſoon as, by the force of imagination, you drive away the fallacious appearance of a concave moon, you reſtore the mountains to their protuberance, and ſink the cavities again below the level of the ſurface. Now, when I ſaw the ſpot lower than the ſhining matter of the ſun, and an extended plane, alſo depreſſed, with ſhelving ſides riſing up to the level, I alſo found that the ſun was convex, and appeared in its natural globular ſtate. Hence I conclude that there could be no deception in thoſe appearances.

How very ill would this obſervation agree with the ideas of ſolid bodies bobbing up and down in a fiery liquid? with the ſmoke of volcanoes, or ſcum upon an ocean? And how eaſily it is explained upon our foregoing theory. The removal of the ſhining atmoſphere, which permits us to ſee the ſun, muſt naturally be attended with a gradual diminution on its borders: an inſtance of a ſimilar kind we have daily before us, when through the opening of a cloud we ſee the ſky, which generally is attended by a ſurrounding hazineſs of ſome ſhort extent; and ſeldom tranſits, from a perfect clearneſs, at once to the greateſt obſcurity.

Aug. 26, 1792. I examined the ſun with ſeveral powers, from 90 to 500. It appears evidently that the black ſpots are the opaque ground, or body of the ſun; and that the luminous part is an atmoſphere, which, being interrupted or broken, gives us a tranſient glimpſe of the ſun itſelf. My 7-feet reflector, which is in high perfection, repreſents the ſpots, as it always uſed to do, much depreſſed below the ſurface of the luminous part.

Sept. 2, 1792. I ſaw two ſpots in the ſun with the naked eye. In the teleſcope I found they were cluſters of ſpots, with many ſcattered ones beſides. Every one of them was certainly below the ſurface of the luminous diſk.

Sept. 8, 1792. Having made a ſmall ſpeculum, merely brought to a perfect figure upon hones without poliſh. I found that, by ſtifling a great part of the ſolar rays, my ob-

ject-ſpeculum would bear a greater aperture; and thus enabled me to ſee with more comfort, and leſs danger. The ſurface of the ſun was unequal; many parts of it being elevated, and others depreſſed. This is here to be underſtood of the ſhining ſurface only, as the real body of the ſun can probably be ſeldom ſeen otherwiſe than in its black ſpots.

It may not be impoſſible, as light is a tranſparent fluid, that the ſun's real ſurface alſo may now and then be perceived; as we ſee the ſhape of the wick of a candle through its flame, or the contents of a furnace in the midſt of the brighteſt glare of it: but this, I ſhould ſuppoſe, will only happen where the lucid matter of the ſun is not very accumulated.

Sept. 9, 1792. I found one of the dark ſpots in the ſun drawn pretty near the preceding edge. In its neighbourhood I ſaw a great number of elevated bright places, making various figures: I ſhall call them *faculæ*, with Hevelius; but without aſſigning to this term any other meaning than what it will hereafter appear ought to be given to it. I ſee theſe faculæ extended, on the preceding ſide, over about 1-6th part of the ſun; but ſo far from reſembling torches, they appear to me like the ſhrivelled elevations upon a dried apple, extended in length, and moſt of them are joined together, making waves, or waving lines.

By ſome good views in the afternoon, I find that the reſt of the ſurface of the ſun does not contain any faculæ, except a few on the following, and equatorial part of the ſun. Towards the north and ſouth I ſee no faculæ: there is all over the ſun a great unevenneſs in the ſurface, which has the appearance of a mixture of ſmall points of an unequal light; but they are evidently an unevenneſs or roughneſs of high and low parts.

Sept. 11, 1792. The faculæ, in the preceding part of the ſun, are much gone out of the diſk, and thoſe in the following are come on. A dark ſpot alſo is come on with them.

Sept. 13, 1792. There are a great number of faculæ on the equatorial part of the ſun, towards the preceding and following parts. I cannot ſee any towards the poles; but a roughneſs is viſible every where.

Sept. 16. 1792. The ſun contains many large faculæ on the following ſide of its equator, and alſo ſeveral on the preceding ſide. I perceive none about the poles. They ſeem generally to accompany the ſpots; and probably, as the faculæ certainly are elevations, a great number of them may occaſion neighbouring depreſſions, that is to ſay, dark ſpots.

The faculæ being elevations, very ſatisfactorily explains the reaſon why they diſappear towards the middle of the ſun, and re-appear on the other margin; for, about the place where we loſe them, they begin to be edge-ways to our view; and if between the faculæ ſhould lie dark ſpots, they will moſt frequently break out in the middle of the ſun, becauſe they are no longer covered by the ſide-views of theſe faculæ.

Sept. 22, 1792. There are not many faculæ in the ſun, and but few ſpots; the whole diſk, however, is very much marked with roughneſs, like an orange. Some of the loweſt parts of the inequalites are blackiſh.

Sept. 23, 1792. The following ſide of the ſun contains many faculæ near the limb. They take up an arch of about 50 degrees. There are likewiſe ſome on the preceding ſide. The north and ſouth is rough as uſual, but differently diſpoſed. The faculæ are ridges of elevations above the rough ſurface.

Feb. 23, 1794. By an experiment I have juſt now tried, I find it confirmed that the ſun cannot be ſo diſtinctly viewed with a ſmall aperture and faint darkening glaſſes, as with a large aperture and ſtronger ones; this latter is the method I always uſe.

One of the black ſpots on the preceding margin, which was greatly below the ſurface of the ſun, had, next to it, a protuberant lump of ſhining matter, a little brighter than the reſt of the ſun.

About all the ſpots, the ſhining matter ſeems to have been diſturbed; and is uneven, lumpy, and zig-zagged in an irregular manner.

I call the ſpots black, not that they are entirely ſo, but merely to diſtinguiſh them; for there is not one of them to-day, which is not partly, or entirely, covered over with whitiſh and unequally bright nebulouſneſs, or cloudineſs. This,

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in many of them, comes near to an extinction of the ſpot; and in others, ſeems to bring on a ſubdiviſion.

Sept. 28, 1794. There is a dark ſpot in the ſun on the following ſide. It is certainly depreſſed below the ſhining atmoſphere, and has ſhelving ſides of ſhining matter, which riſe up higher than the general ſurface, and are brighteſt at the top. The preceding ſhelving ſide is rendered almoſt inviſible by the overhanging of the preceding elevations, while the following is very well expoſed; the ſpot being apparently ſuch in figure as denotes a circular form viewed in an oblique direction.

Near the following margin are many bright elevations, cloſe to viſible depreſſions. The depreſſed parts are leſs bright than the common ſurface.

The penumbra, as it is called, about this ſpot, is a conſiderable plane, of leſs brightneſs than the common ſurface, and ſeems to be as much depreſſed below that ſurface as the ſpot is below the plane.

Hence, if the brightneſs of the ſun is occaſioned by the lucid atmoſphere, the intenſity of the brightneſs muſt be leſs where it is depreſſed; for light, being tranſparent, muſt be the more intenſe the more it is deep.

Oct. 12, 1794. The whole ſurface of the ſun is diverſified by inequality in the elevation of the ſhining atmoſphere. The loweſt parts are every where darkeſt; and every little pit has the appearance of a more or leſs dark ſpot.

A dark ſpot, which is on the preceding ſide, is ſurrounded by very great inequalities in the elevation of the lucid atmoſphere; and its depreſſion below the ſame is bounded by an immediate riſing of very bright light.

Oct. 13, 1794. The ſpot in the ſun I obſerved yeſterday is drawn ſo near the margin, that the elevated ſide of the following part of it hides all the black ground, and ſtill leaves the cavity viſible, ſo that the depreſſion of the black ſpots, and the elevation of the faculæ, are equally evident.

[To be continued.]

III. *On the Production of Caſt Iron, and the Operations of the Blaſt-Furnace. By Mr.* DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

IT was my wiſh, in the papers preceding this communication, to convey a clear and competent idea of the nature of iron as a metal; as alſo of ores, and iron-ſtones in general. I have endeavoured to explain, upon principles grounded on experiment, the chief agents of change in the fmelting operation, ſo far as they affect the quality of the materials prepared. I have aimed at perſpicuity rather than minuteneſs, which often becomes tedious; and although the facts may not be conveyed in a ſtyle ſo popular as communications, which profeſs a conveyance of practical truths, generally are, yet, I truſt, this will form no real bar to their utility. When facts are to be learned, and principles ſatisfactorily explained, it is ſurely beſt to begin by the examination of ſimple cauſes; and tracing their agency according to their quantities, relative proportions, and affinities. Theſe facts will undoubtedly, in the end, be more radically underſtood by uſing ſuch phraſes, or ſigns, as denote in each a reſpective quality affixed in conſequence of a knowledge of its properties and effects. In ſhort, if the language and reaſoning of our manufactories is ever to become ſcientific; if philoſophy and chemiſtry are ever to become of general utility in the perfection of any branch; then thoſe truths which conſtitute the foundation of all ſcience are not to be rejected, though clothed in a dreſs which at firſt ſight appears diſcordant to our habits, or burdenſome to our memory.

The operations I am about to deſcribe have never as yet received any explanation conſonant to true philoſophy or chemical facts; yet there are few which preſent a more beautiful chain of affinities, decompoſition, and recombination, than the manufacture of iron in all its various ſtages. An extenſive foundry is a laboratory fraught with phenomena of the moſt intereſting nature in chemiſtry and natural philoſophy: are we not then juſtly ſurpriſed to find that prejudice ſtill

ſtill reigns there; and that the curious manipulations of theſe regions are ſtill ſhrouded with error and miſconception; as if their dingy ſtructure forbade the entrance of genius, or conſigned her laborious unlettered ſons to an endleſs ſtretch of mental obſcurity?

The plate of the blaſt-furnace, given in the preceding Number, having a full deſcription appropriated to it, I ſhall proceed to detail the train of preparation neceſſary before the furnace is brought to produce good melting iron.

The furnace being finiſhed, the bottom and ſides of it, for two feet up the ſquare funnel, receive a lining of common bricks upon edge, to prevent the ſtone from ſhivering or mouldering when the fire comes in contact with it. On the front of the furnace is erected a temporary fire-place, about four feet long, into the bottom of which are laid correſponding bars. The ſide-walls are made ſo high as to reach the under-ſurface of the tymp-ſtone; excepting a ſmall ſpace, which afterwards receives an iron plate of 1½ inches thickneſs, by way of a cover: this alſo preſerves the tymp-ſtone from any injury it might ſuſtain by being in contact with the flame. A fire is now kindled upon the bars, and is fed occaſionally with ſmall coals. As the whole cavity of the furnace ſerves as a chimney for this fire, the draught in conſequence is violent, and the body of heat carried up is very conſiderable. In the courſe of three weeks the furnace will thus become entirely free from damp, and fit for the reception of the materials: when this is judged proper the fire-place is removed, but the interior bricks are allowed to remain till the operation of blowing commences. Some looſe fuel is then thrown upon the bottom of the furnace, and a few baſkets of cokes are introduced; theſe are allowed to become thoroughly ignited before more are added. In this manner the furnace is gradually filled; ſometimes entirely full, and at other times 5-8ths or 3-4ths full. The number of baſkets full depend entirely upon the ſize of the furnace: that in the plate will contain 900 baſkets. If the coal is ſplint, the weight of each baſket-full will be nearly 110 lb. × 900 = 99,000 lb. cokes. As this quality of cokes is made with a loſs of nearly 50 *per cent.* the original weight in raw

coals will be equal to 198,000 lb. When we reflect that this vaſt body of ignited matter is replaced every third day, when the furnace is properly at work, a notion may be formed of the immenſe quantity of materials requiſite, as alſo the conſequent induſtry exerted to ſupply one or more furnaces for the ſpace of one year.

When the furnace is ſufficiently heated throughout, ſpecific quantities of cokes, iron-ſtone, and blaſt-furnace cinders are added: theſe are called charges. The cokes are commonly filled in baſkets, which, at all the various iron-works, are nearly of a ſize. The weight of a baſket, however, depends entirely upon the nature and quality of the coal, being from 70 to 112 lb. each *. The iron-ſtone is filled into boxes, which, when moderately heaped, contain 56 lb. of torrefied iron-ſtone; they often exceed this when the ſtone has been ſeverely roaſted. The firſt charges which a furnace receives, contain but a ſmall proportion of iron-ſtone to the weight of cokes: this is afterwards increaſed to a full burden, which is commonly 4 baſkets cokes, 320 lb.; 2 boxes iron-ſtone, 112 lb.; 1 box blaſt-furnace cinders, 60 or 70 lb. † At new works, where theſe cinders cannot be obtained, a ſimilar quantity of limeſtone is uſed.

The deſcent of the charge, or burden, is facilitated by opening the furnace below two or three times a-day, throwing out the cold cinders, and admitting, for an hour at a time, a body of freſh air. This operation is repeated till the approach of the iron-ſtone and cinder, which is always announced by a partial fuſion, and the dropping of lava through the iron bars, introduced to ſupport the incumbent materials while thoſe on the bottom are carried away. The filling above is regularly continued, and when the furnace at the

* This ſame variety in the coal renders it almoſt impoſſible to treat under one deſcription, to give a juſt idea of the proportions uſed at various blaſt-furnaces: to avoid being too diffuſe, I ſhall confine my deſcription connected with a coal of a medium quality, or a mixture of ſplint and free-coal, a baſket of which will weigh from 78 lb. to 84 lb.

† A preference at firſt is always given to blaſt-furnace cinders in place of lime; being already vitrified, they are of much eaſier fuſion, and tend to preſerve the ſurface of the hearth by glazing it over with a black vitrid ſtuff.

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top has acquired a conſiderable degree of heat, it is then judged time to introduce the blaſt; the preparations neceſſary for which are the following:—

The dam-ſtone is laid in its place firmly imbedded in fire-clay; the dam-plate is again imbedded on this with the ſame cement, and is ſubject to the ſame inclination. On the top of this plate is a ſlight depreſſion, of a curved form, towards that ſide fartheſt diſtant from the blaſt, for the purpoſe of concentrating the ſcoria, and allowing it to flow off in a connected ſtream, as it tends to ſurmount the level of the dam. From this notch to the level of the floor a declivity of brick-work is erected, down which the ſcoria of the furnace flows in large quantities. The opening betwixt the dam and ſide-walls of the furnace, called the *fauld*, is then built up with ſand, the looſe bricks are removed, and the furnace bottom is covered with powdered lime or charcoal-duſt. The ignited cokes are now allowed to fall down, and are brought forward with iron bars nearly to a level with the dam. The ſpace between the ſurface of the cokes and the bottom of the tymp-plate is next rammed hard with ſtrong binding ſand; and theſe cokes, which are expoſed on the outſide, are covered with coke-duſt. Theſe precautions being taken, the tuyere-hole is then opened and lined with a ſoft mixture of fire-clay and loam: the blaſt is commonly introduced into the furnace at firſt with a ſmall diſcharging-pipe, which is afterwards increaſed as occaſion may require. In two hours after blowing, a conſiderable quantity of lava will be accumulated; iron bars are then introduced, and perforations made in the compreſſed matter at the bottom of the furnace; the lava is admitted to all parts of the hearth, and ſoon thoroughly heats and glazes the ſurfaces of the fire-ſtone. Shortly after this it riſes to a level with the notch in the dam-plate, and by its own accumulation, together with the forcible action of the blaſt, it flows over. Its colour is at firſt black; its fracture denſe, and very ponderous; the form it aſſumes in running off is flat and branched, ſometimes in long ſtreams, and at other times leſs extenſive. If the preparation has been well conducted, the colour of the cinder will ſoon change to white; and the metal, which in the ſtate of an oxyde formerly coloured

coloured it, will be left in a diſengaged ſtate in the furnace. When the metal has riſen nearly to a level with the dam, it is then let out by cutting away the hardened loam of the fauld, and conveyed by a channel, made in ſand, to its proper deſtination; the principal channel, or runner, is called the *ſow*, the lateral moulds are called the *pigs*.

In ſix days after the commencement of blowing, the furnace ought to have *wrought herſelf clear*, and have acquired capacity ſufficient to contain from 5000 to 7000 weight of iron. The quality ought alſo to be richly carbonated, ſo as to be of value and eſtimation in the pig-market. At this period, with a quality of coal as formerly mentioned, the charge will have increaſed to the following proportions:—5 baſkets cokes, 400lb.; 6 boxes iron-ſtone, 336lb.; 1 box limeſtone, 100lb.

An analyſis of the ſmelting operation, and the tendency which the individual agents have to produce change in the quality and quantity of the iron, come next under conſideration. Let us, however, firſt notice the characteriſtic features exhibited by the different kinds of iron while in fuſion, whereby the quality of the metal may be juſtly defined.

When fine (No. 1.) or ſupercarbonated crude iron is run from the furnace, the ſtream of metal, as it iſſues from the fauld, throws off an infinite number of brilliant ſparkles of carbon. The ſurface is covered with a fluid pellicle of carburet of iron, which, as it flows, rears itſelf up in the moſt delicate folds: at firſt the fluid metal appears like a denſe, ponderous ſtream, but, as the collateral moulds become filled, it exhibits a general rapid motion from the ſurface of the pigs to the centre of many points; millions of the fineſt undulations move upon each mould, diſplaying the greateſt nicety and rapidity of movement, conjoined with an uncommonly beautiful variegation of colour, which language is inadequate juſtly to deſcribe. Such metal, in quantity, will remain fluid for twenty minutes after it is run from the furnace, and when cold will have its ſurface covered with the beautiful carburet of iron, already mentioned, of an uncommonly rich and brilliant appearance. When the ſurface of the metal is not carbureted, it is ſmooth like forged iron, and

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always convex. In this ſtate iron is too rich for melting without the addition of coarſe metal, and is unfit to be uſed in a cupola furnace for making fine caſtings, where thinneſs and a good ſkin are requiſite.

No. 4, or oxygenated crude iron, when iſſuing from the blaſt-furnace, throws off from all parts of the fluid ſurface a vaſt number of metallic ſparks: they ariſe from a different cauſe than that exerted in the former inſtance. The extreme privation of carbon renders the metal ſubject to the combination of oxygen ſo ſoon as it comes into contact with atmoſpheric air. This truth is evidently manifeſted by the ejection of ſmall ſpherules of iron from all parts of the ſurface: the deflagration does not, however, take place till the globule has been thrown two or three feet up in the air; it then inflames and ſeparates, with a ſlight hiſſing exploſion, into a great many minute particles of brilliant fire. When theſe are collected they prove to be a true oxyde of iron, but ſo much ſaturated with oxygen as to poſſeſs no magnetic obedience. The ſurface of oxygenated iron, when running, is covered with waving flakes of an obſcure ſmoky flame, accompanied with a hiſſing noiſe; forming a wonderful contraſt with the fine rich covering of plumbago in the other ſtate of the metal, occaſionally parting and exhibiting the iron in a ſtate of the greateſt apparent purity, agitated in numberleſs minute fibres, from the abundance of the carbon united with the metal.

When iron thus highly oxygenated comes to reſt, ſmall ſpecks of oxyde begin to appear floating upon the ſurface: theſe increaſe in ſize; and when the metal has become ſolid, the upper ſurface is found entirely covered with a ſcale of blue oxyde of various thickneſſes, dependent upon the ſtage of oxygenation or extreme privation of carbon. This oxyde, in common, contains about 15 *per cent.* of oxygen, and is very obedient to the magnet. In place of a dark blue ſmooth ſurface, convex and richly carbonated, the metal will exhibit a deep, rough, concave face, which, when the oxyde is removed, preſents a great number of deep pits. This iron in fuſion ſtands leſs convex than carbonated iron, merely becauſe it is leſs ſuſceptible of a ſtate of extreme diviſion; and

indeed it ſeems a principle in all metallic fluids, that they are convex in proportion to the quantity of carbon with which they are ſaturated. This iron flows dead and ponderous, and rarely parts in ſhades but at the diſtance of ſome inches from each other.

This is a ſlight ſketch of the appearance of the two extreme qualities of crude, or pig-iron, when in a ſtate of fuſion. According to the diviſion formerly made, there ſtill remains two intermediate ſtages of quality to be deſcribed: theſe are, carbonated and carbo-oxygenated iron; that is, No. 2 and 3 of the manufacturers. Carbonated iron exhibits, like No. 1, a beautiful appearance in the runner and pig. The breakings of the fluid, in general, are leſs fine; the agitation leſs delicate; though the diviſion of the fluid is equal, if not beyond that of the other. When the internal ebullition of the metal is greateſt, the undulating ſhades are ſmalleſt and moſt numerous: ſometimes they aſſume the ſhape of ſmall ſegments; ſometimes fibrated groups; and at other times minute circles, of a mellower colour than the ground of the fluid. The ſurface of this metal, expoſed to external air, when cooling is generally ſlightly convex, and full of punctures: theſe, in iron of a weak and fuſible nature, are commonly ſmall in the diameter, and of no great depth. In ſtrong metal, the punctures are much wider and deeper. This criterion, however, is not infallible, when pig-iron of different works is taken collectively. At each individual work, however, that iron will be ſtrongeſt whoſe honeycombs are largeſt and deepeſt.

Carbo-oxygenated, or No. 3, pig-iron, runs ſmoothly, without any great degree of ebullition or diſengagement of metallic ſparks. The partings upon its ſurface are longer, and at greater diſtances from each other than in the former varieties; the ſhape they aſſume is either elliptical, circular, or curved. In cooling, this metal acquires a conſiderable portion of oxyde; the ſurface is neither markedly convex nor concave; the punctures are leſs, and frequently vaniſh altogether. Their abſence, however, is no token of a ſmooth face ſucceeding: in qualities of crude iron oxygenated beyond this, I have already mentioned that a concave ſurface

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is the conſequence of the extreme abſence of carbon; and that, in proportion as this principle is abſent, the ſurface of the iron acquires roughneſs and aſperity.

It may perhaps be proper here to mention, once for all, that although, for convenience, the manufacturer has, from a juſt eſtimation of the value of the metal in a ſubſequent manufacture, affixed certain numbers for determinate qualities of iron, yet it is difficult to ſay at what degree of ſaturation of carbon each reſpective term commences: ſuffice it then to ſay, that the two alterative principles, oxygen and carbon, form two diſtinct claſſes, that in which oxygen predominates, and that in which carbon predominates; the latter comprehends No. 1 and 2 of the manufacturers, the former includes oxygenated, white and mottled; and the equaliſation of theſe mixtures form, as has already been noticed, the variety of carbo-oxygenated crude iron.

I ſhall now obſerve ſome things relative to the various faces which crude iron aſſumes. No. 1 and 2, with their intermediate qualities, poſſeſs ſurfaces more or leſs convex, and frequently with thin bliſters: this we attribute to the preſence of carbon, which being plentifully interſperſed betwixt and throughout the particles of the metal, the tendency which the iron has to ſhrink in cooling is entirely done away; it tends to diſtend the aggregate of the maſs, and to give a round face, by gradually elevating the central parts of the ſurface, which are always laſt to loſe their fluidity.

Again, that quality of iron known by the name of No. 3, or carbo-oxygenated, is moſt commonly found with a flat ſurface. If we ſtill farther trace the appearance of the ſurface of pig-iron, when run from the furnace, we ſhall find No. 4, either with a white or mottled fracture, poſſeſſed of concave faces rough and deeply pitted. Beyond this it may be imagined that every degree of further oxygenation would be productive of a ſurface deeper in the curve, and rougher, with additional aſperities. The contrary is the caſe: when crude iron is ſo far debaſed as to be run from the furnace in clotted lumps highly oxygenated, the ſurface of the pigs is found to be more convex than that of No. 1 iron; but then

the fracture of ſuch metal preſents an impure maſs covered on both faces with a mixture of oxydated iron, of a blueiſh colour, nearly metallic. In ſhort, this quality of iron is incapable of receiving ſuch a degree of fluidity as to enable us to judge whether the convexity of its ſurface is peculiar to its ſtate, or is owing to its want of diviſion as a fluid, whereby the gradual conſolidation of the metal is prevented.

Theſe features ſufficiently diſtinguiſh betwixt the various qualities of crude iron after they are obtained from the blaſt-furnace: there are, however, criterions not leſs infallible, whereby we can prejudge the quality of the metal many hours before it is run from the furnace. Theſe are the colour and form of the ſcoria, the colour of the vitrid cruſt upon the working bars, and the quantity of carburet which is attached to it. The variety of colour and form in the cinder almoſt univerſally indicate the quality of the metal on the hearth. Hence, from a long courſe of experience, have ariſen the following denominations: "Cinder of ſulphury iron;" "Cinder of No. 1, No. 2, and No. 3;" and "Cinder of ballaſt iron." Although at different works, from local circumſtances, the ſame kind of ſcoria may not indicate preciſely the ſame quality of iron, yet the difference is ſo ſmall that the following deſcription of the various cinders may convey a very juſt idea of their general appearance.

When the ſcoria is of a whitiſh colour and ſhort form, branching from the notch of the dam, and emitting from its ſtream beautiful ſparks of ignited carbon, reſembling thoſe ejected from a crucible of caſt ſteel in fuſion, expoſed to external air, or to the combuſtion of fine ſteel filings in a white flame; if, when iſſuing from the orifice of the furnace, it is of the pureſt white colour, poſſeſſing no tenacity, but in a ſtate of the greateſt fluid diviſion, and, when cold, reſembles a maſs of heavy torrefied ſpar, void of the ſmalleſt vitrid appearance, hard and durable, it is then certain that the furnace contains *ſul hury iron*, i. e. ſuper-carbonated iron. At blaſt-furnaces, where a great quantity of air is thrown in *per* minute, ſuper-carbonated crude iron will be obtained with a cinder of a longer form, with a rough flinty fracture towards the outſide of the column.

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That cinder which indicates the preſence of carbonated iron in the hearth of the furnace, forms itſelf into circular compact ſtreams, which become conſolidated and inſerted into each other; theſe are in length from three to nine feet. Their colour, when the iron approaches the firſt quality, is a beautiful variegation of white and blue enamel, forming a wild profuſion of the elements of every known figure; the blues are lighter or darker according to the quantity of the metal and the action of the external air while cooling. When the quality of the pig-iron is ſparingly carbonated, the blue colour is leſs vivid, leſs delicate; and the external ſurface rougher, and more ſullied with a mixture of colour. The ſame ſcoria, when fuſed in veſſels which are allowed to cool gradually, parts with all its variety of light and ſhade, and becomes of a yellowiſh colour, ſometimes nearly white when the quantity of incorporated metal has been ſmall.

The cinder which is emitted from the blaſt-furnace when carbo-oxygenated (or No. 3,) iron is produced, aſſumes a long zig-zag form. The ſtream is ſlightly convex in the middle; broad, flat, and obliquely furrowed towards the edges. The end of the ſtream frequently rears itſelf into narrow tapered cones, to the height of ſix or eight inches: theſe are generally hollow in the centre, and are eaſily demoliſhed, owing to their exceſſive brittleneſs. The colour of this lava is very various; for the moſt part it is pale yellow mixed with green. Its tenacity is ſo great, that if, while fluid, a ſmall iron hook is inſerted into it at a certain degree of heat, and then drawn from it with a quick but ſteady motion, 20 to 30 yards of fine glaſs thread may be formed with eaſe. If the colours are vivid and variegated, the thread will poſſeſs, upon a minute ſcale, all the various tints of colouring which is found in the columnar maſs. When by accident a quantity of this lava runs back upon the diſcharging-pipe, it is upon the return of the blaſt impelled with ſuch velocity as to be blown into minute delicate fibres, ſmaller than the moſt ductile wire; at firſt they float upon the air like wool, and when at reſt very much reſemble that ſubſtance.

The preſence of oxygenated crude iron (No. 4,) on the furnace-

furnace-hearth, is indicated by the lava reſolving itſelf into long ſtreams, ſometimes branched, ſometimes columnar, extending from the notch to the loweſt part of the declivity; here it commonly forms large, flat, hollow cakes, or inclines to form conical figures: theſe are, however, ſeldom perfect; for the quantity of fluid lava, conveyed through the centre of the column, accumulates faſter than the external ſides of the cone are conſolidated; and thus, when the ſtructure is only half finiſhed, the ſmall crater vomits forth its ſuperabundant lava, and is demoliſhed. The current of ſuch lava falls heavily from the dam as if ſurcharged with metal, and emits dark red ſparks reſembling the agitation of ſtraw embers. Its colour is ſtill more varied than the former deſcriptions of ſcoriæ, and is found changing its hues through a great variety of greens ſhaded with browns. Another variety of ſcoria, which indicates the ſame quality of iron, aſſumes a ſimilar form; but has a black ground colour mixed with browns, or is entirely black. When the latter colour prevails, the texture of the cinder becomes porous; the quantity of iron left is now very conſiderable, and ſuch as will be eaſily extracted in the aſſay-furnace with proper fluxes. In caſes of total derangement in the furnace, the ſcoria will ſtill retain this black colour, although the quantity of metal may amount to 25 *per cent.*; the fracture, however, becomes denſe, and its ſpecific gravity increaſes in proportion to the quantity of metal it holds incorporated.

The next ſource of information, as to the quality of the iron in the furnace, is to be got from the colour of the ſcoria upon the working bars, which are from time to time inſerted to keep the furnace free from lumps, and to bring forward the ſcoria. When ſuper-carbonated crude iron is in the hearth, the vitrid cruſt upon the bars will be of a black colour and ſmooth ſurface, fully covered with large and brilliant plates of plumbago.

As the quality of the metal approaches to No. 2 (carbonated), the carburet upon the ſcoria decreaſes both in point of quantity and ſize.

When carbo-oxygenated iron (No. 3) is in the furnace, the working bars are always coated with a lighter co-

loured ſcoria than when the former varieties exiſt; a ſpeck of plumbago is now only found here and there, and that of the ſmalleſt ſize. When the quality of the metal is oxygenated (No. 4.), not only have the plates of carburet diſappeared, but alſo the coally colour on the external ſurface of the ſcoria; what now attaches to the bars, is nearly of the ſame nature and colour as the lava emitted at the notch of the dam.

Theſe criterions are infallible; for, as the fuſibility or carbonation of the metal is promoted in a direct ratio to the comparative quantity of the coally principle preſent in the furnace, ſo in the ſame proportion will the vitrid cruſt encircling the working bars exhibit the preſence of that principle in the furnace.

IV. *Agenda, or a Collection of Obſervations and Reſearches, the Reſults of which may ſerve as the Foundation for a Theory of the Earth.* *By* M. De Saussure.

[Continued from Page 29.]

CHAP. XXII.

Errors to be avoided in Obſervations reſpecting Geology.

1. THERE are ſome errors into which people may readily fall when they have not had long experience in any given kind of obſervation, and againſt which it is of importance to put beginners at leaſt on their guard.

2. One may be readily deceived in regard to the relative diſtances of remote objects. All the ſtars and planets appear to be at an equal diſtance from us. Diſtant mountains all appear to be in the ſame plain. Thus thoſe which are ſituated very far behind the reſt, ſeem to form one body with them; ſo that people believe they ſee continued and uninterrupted chains when there are really none, and where the mountains, on the contrary, are inſulated.

The abſolute diſtance of objects, even when not very remote, is equally difficult to be aſcertained on high mountains,

tains, where the tranſparency of the air, and the abſence of vapours, deſtroy the aërial perſpective. I have often imagined that I had only two or three hundred ſteps to make in order to reach a ſummit, the diſtance of which from me was more than a league in a ſtraight line.

3. There are a great many errors in regard to ſtrata. Their great thickneſs may make one believe that there are none where they really exiſt. In the like manner, if the vertical ſtrata, or thoſe only very much inclined, preſent their planes to the eye of the obſerver, he will think he ſees ſhapeleſs and indiviſible maſſes; while, if their ſections were ſeen, their diviſions would readily be diſtinguiſhed. A mountain then muſt be ſeen under aſpects that interſect each other at right angles before we can pronounce that it is not divided by ſtrata.

4. At other times accidental fiſſures, but produced however by a cauſe which is common to them, exhibit the appearance of ſtrata when there are none; or when, if there are, their ſituation is very different from that of thoſe ſtrata. It is the internal tiſſue of the ſtone only which in many caſes can determine whether the diviſions obſerved are the interſtices between ſtrata or mere fiſſures; becauſe the ſtrata are conſtantly parallel to the internal laminæ, or ſchiſtous texture of the ſtone. Cryſtals, the lamellated texture of which may ſometimes be confounded with a ſchiſtous texture, may afford an exception to this rule, by preſenting laminæ perpendicular to the planes of the ſtrata; but it is not difficult to diſtinguiſh them.

5. One may alſo form an erroneous opinion reſpecting the direction of a mountain, or of its ſtrata, when the eye is not ſituated in their prolongation, or at leaſt near it.

6. The apparent ſituation of the ſtrata may alſo lead into an error. They appear horizontal even when they are very much inclined, and when they are not ſeen but in a ſection formed by a plane parallel to the common ſection of their planes with the horizon. It is impoſſible to judge of their inclination, and to meaſure it with certainty, but on a ſection perpendicular to the common ſection, which I have juſt mentioned.

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6. A. The greateſt error, however, is that which may be committed in regard to the ſuper-poſition of ſtrata. I have often ſeen novices in the ſtudy of mountains believe that one ſtratum repoſed on another; one of granite, for example, on one of ſlate; becauſe they found ſlate at the bottom of the mountain, and granite at the top; while the ſlate was only laid againſt the baſe of the mountain, and the granite, on the other hand, was ſunk in the earth far below the ſlate. We muſt not then ſay, that a ſtratum is ſituated below another, but when we really ſee it extending itſelf below it.

7. And even when we diſtinctly ſee a rock placed above another, we muſt examine whether that which is uppermoſt does not occupy that ſituation accidentally; whether it has not ſlipped, or rólled down, from a more elevated mountain; and, in the laſt place, though they may be cloſely connected, one muſt examine whether their preſent ſituation is really the ſame in which they were formed, and whether they have not been reverſed, and united accidentally in a ſituation contrary to that of their original formation.

8. One is frequently deceived, alſo, in regard to the nature of ſtones and of mountains. Though a well-accuſtomed eye may often judge at ſome, and even a conſiderable diſtance, of the kind of ſtone of which a mountain is compoſed, ſuch judgment is however often erroneous: mountains of granite, or *gneiſs*, tender and deſtructible, often aſſume, at a diſtance, the round form of ſecondary mountains; ſometimes, alſo, mountains of calcareous ſtone, hard of their kind, and in ſtrata either vertical or very much inclined, preſent the bold forms, the peaks, and ſharp-angled indentations of the granite ſummits.

9. People are often deceived even on a near view. A ſtone may have a foreign covering of mica, for example, while the interior part is of a very different nature.

10. Effervéſcence with the nitrous acid is commonly conſidered as a certain character of calcareous ſtone; but this character may be deceptive, ſince bárytes and magneſia effervéſce alſo*: and we muſt not conſider it enough to touch

* And, on the other hand, there are calcareous ſtones which do not effervéſce.

a ſtone with the nitrous acid, or to let fall a drop of the acid on its ſurface, ſince the abſorbing earth, whatever it is, may be only diſſeminated between argillaceous or ſiliceous particles. We muſt therefore immerſe a fragment of the ſtone in a quantity of the acid ſufficient to diſſolve it entirely, if it be wholly ſoluble, and obſerve whether there remains any reſiduum that withſtands ſolution.

11. The action of the air and of meteors often gives foſſils appearances abſolutely different from thoſe which they had before they were ſubjected to it. We muſt not then be ſatisfied with a ſuperficial examination: we muſt found the rocks to the quick where the action of meteoric agents has not penetrated.

12. People are often deceived, alſo, in conſidering compound ſtones as ſimple ſtones, when the compoſition of them does not manifeſt itſelf on the firſt view, either on account of the ſmallneſs of their compoſing parts, or becauſe ſome of theſe parts are each incloſed ſeparately in a covering which conceals the interior of them. One may guard againſt this error by obſerving the foſſil in the ſun with ſtrong magnifying glaſſes, after having moiſtened its ſurface with water or the nitrous acid, and ſtill better by expoſing it gradually to the flame of the blow-pipe.

13. People are often deceived in regard to cryſtalliſation, either in the true form of the cryſtals, or, above all, in taking for real cryſtals paraſite cryſtals, or ſuch as have been formed in the moulds made by cryſtals of another kind. Thus we ſee cryſtals of quartz, petro-ſilex, and jaſper, formed in the moulds made by calcareous cryſtals, and which have aſſumed the form of the latter.

14. In regard to errors occaſioned by ignorance of the diſtinctive characters of foſſils, and of the names proper for them, the only means of avoiding ſuch errors is to ſtudy with care good authors; and, above all, collections formed, or at leaſt arranged and titled, by able mineralogiſts.

15. But when the ſlighteſt doubt is entertained in regard to the denominations which ought to be given to any foſſil, an exact deſcription muſt be made either of its external characters or its moſt ſtriking phyſical properties, ſuch as weight

and

and ſolubility *. If this deſcription is well drawn up, the error reſpecting the name may be rectified, and the obſervation will not be loſt, as it would be were there any reaſon to ſuſpect the juſtneſs of the denomination, and no means of correcting it by a deſcription †.

16. When the characters of a foſſil give it ſuch a likeneſs to another that it is found near the limits which ſeparate the genera or ſpecies of theſe two foſſils, we muſt follow the example of Werner and his diſciples, by marking that this foſſil is intermediary, or forms a tranſition from the one ſpecies to the other. For if we ſhould aſcribe it excluſively to the genus A, without noting the characters which bring it near to the genus B, another obſerver, on ſeeing the ſame foſſil, might refer it to the genus B, and no one could know which of them was deceived.

17. People are often deceived alſo by mixing opinion with obſervation, and giving the former for the latter; as when people aſſert, that they have ſeen veſtiges of extinguiſhed volcanoes, becauſe they have ſeen black or porous ſtones, or ſtones of a priſmatic form, without deigning to deſcribe them with care, but by qualifying them merely as *lava* or *baſaltes*.

18. In the laſt place, a very frequent ſource of error is, too great a confidence in the fidelity of one's memory, or in the juſtneſs of one's firſt obſervations. Theſe two kinds of confidence go often hand in hand; and people cannot guard againſt the errors, which are the conſequence of them, but by noting down, on the ſpot, all obſervations to which any importance is attached, eſpecially if they are a little complex, and carry away ſpecimens, with their characters carefully marked upon them, of the objects that are the ſubject of

* Hardneſs, refrangibility, electricity, &c. H.

† A perſon now dead, who in his time was conſidered as a mineralogiſt, wrote to me that he had found marine ſhells in granite. I begged him to give me an exact deſcription of the ſtone which he called *granite*. He did ſo; but I perceived that the ſtone was a free-ſtone or ſand-ſtone, and the ſpecimens he afterwards ſent me proved that I was not deceived. We may here recollect Recupero's pyrites of Ætna. The errors of this kind, ariſing from falſe denominations, are innumerable; for an exact knowledge of mineral ſubſtances is more difficult to be obtained, and more rare, than is generally imagined.—Note of the AUTHOR.

theſe obſervations; for it is not ſpecimens of rare objects merely that ſhould be collected. The end, indeed, of the geological obſerver is, not to form a cabinet of curioſities, but he muſt carry away fragments of things apparently the moſt common, when an exact determination of their nature may be intereſting to theory. People may thus employ, with advantage, the means of confirming or rectifying their firſt obſervations, and of making profound reſearches and compariſons impoſſible to be made on the ſpot *.

[To be continued.]

V. *Obſervations on Animal Electricity, and particularly that called Spontaneous.* By J. J. Hemmer.

[Concluded from Page 7.]

IN a letter, dated June 21, 1787, which I received from M. De Sauſſure, he confeſſes that he had not made any farther experiments on animal electricity; and that he did not know whether any had been made by others. As I was convinced, however, that a complete knowledge of that electricity which is produced in the human body by the friction of the clothes, as well as ſpontaneouſly, might be of great

* We think it our duty to ſubjoin here ſome advice to travellers in regard to the queſtions which they may aſk in the different towns.

Whence do they procure the materials proper for building; ſuch as lime, plaſter, tiles, ſlate, ſtones of different kinds, and ſand? Do they burn turf or coal; and where are they found? Where do they procure their potter's clay, fuller's earth, the clay uſed for refining ſugar, their whetſtones and millſtones? To obſerve with what the ſtreets are paved; of what ſtone the ſteps of ſtairs are formed; marks for boundaries, &c.; and to learn from what place they are brought. To aſcertain whether wells or the foundation of houſes are dug; and whether there are in the neighbourhood any ravines or precipices. Theſe queſtions will ſerve to facilitate the means of obſerving the nature of the ground, by pointing out the natural or artificial excavations that may exiſt in the neighbourhood, or which ought to be viſited. For the ſame reaſon it is proper to examine the ſhores of rivers. It will be of uſe alſo to take a general view of the country from the tops of towers and of the higheſt ſteeples. It will be of ſome importance alſo to enquire, in the country, whether the inhabitants make uſe of lime, marl, plaſter, coal, earth, or turf-aſhes, for manuring their land; and from what places theſe ſubſtances are procured. C.

utility

utility both to the ſcience of electricity in general, and particularly to medical electricity, I reſolved to make reſearches on this ſubject by means of experiments. The method I employed was as follows:—In order that I might examine the electricity of my own body, I inſulated myſelf on a board which ſtood on glaſs feet. I then touched, for a determined time, (at firſt commonly half a minute, afterwards only a moment,) the plate of my condenſer, which I have deſcribed in another place*; I then applied the condenſer to a Cavallo's highly ſenſible electrometer, as improved by M. De Sauſſure, and, by means of a glaſs tube rubbed with a piece of woollen cloth, examined the kind of electricity when announced by the diverging threads. The electricity of the plate of the condenſer or electrometer, which correſponded with that of the body, I marked with + when poſitive, with — when negative, and with o when none was preſent. I made my experiments on the 21ſt of February 1786; and ſince that period have repeated them, not only on myſelf, but on other perſons, both male and female, of various ages and different conſtitutions, when they were in motion, or at reſt; dreſſed and undreſſed; when tired, and in good ſpirits; hot or cold; faſting or full; ſleeping or awake; at different temperatures of the weather and of the apartment, &c.

Reſults of the Experiments.

1. Animal electricity is common to all men; for, as I found it, in the courſe of my experiments, on thirty perſons of both ſexes and all ages, and of every habit of body, I can with juſtice conclude on its being general.

2. Animal electricity is different in different men, at the ſame place and at the ſame time; not only in ſtrength, but alſo in the kind; being in many weak, and others ſtrong; in ſome poſitive, and in ſome negative.

3. This difference of electricity is often obſerved in different perſons, when, beſides time and place, the other circumſtances appear to be all equal.

4. The ſtrength and kind of this electricity are often dif-

* See *Crell's Journal der Phyſik*, Vol. II. Part II. p. 110.

ferent;

ferent; not only in different men, but alſo in the ſame perſon. In 2422 experiments, in which I examined my own electricity, I found it 1252 times poſitive, 771 times negative, and 399 times 0. The electricity of my maid, in 94 experiments, was, on the other hand, 19 times poſitive, 33 times negative, and 42 times 0.

5. It often happens, that, during the experiment, the electricity changes; that is, from poſitive to negative, or from ſtronger poſitive to weaker, and at laſt becomes 0. It then paſſes from the laſt to weak negative, and gradually to ſtronger.

6. It happens alſo, not unfrequently, that the electricity, at the commencement of the experiment, is ſtrongly poſitive or negative, but paſſes afterwards to the oppoſite kind with the like force, which continues for a conſiderable time.

7. Notwithſtanding this wonderful and almoſt continual alternation, animal electricity appears to be naturally poſitive. As the electric matter is diffuſed throughout all nature, and adheres, in a fixed form, to no body, all the articles of our nouriſhment contain each their ſhare of it. The electric fluid is diſengaged, and diffuſed with the blood and juices through the body; from which it is conveyed off, after being accumulated in it, by the pores of the ſkin and other ducts, when there is no interruption in them. This is confirmed by experience; for when animal electricity is examined in a body expoſed to no violent exertions, as when a perſon ſits or repoſes, and when there is conſequently no great loſs of heat, it is commonly poſitive. Thus the electricity of my body, which I examined while ſitting at reſt, when the natural heat of the body was not diſturbed, appeared, in 332 experiments poſitive, in 14 negative, and in 10 was 0.

8. When animal electricity, therefore, is 0, or negative, the body muſt be in an unnatural ſtate *(ſtatu violento)*.

9. Cold changes natural or poſitive animal electricity into the oppoſite kind, or at leaſt leſſens it. In 60 experiments I made on myſelf after coming from a cold air, the temperature of which was at the freezing point or below it, my electricity was 38 times negative, 7 times 0, and 15 times poſitive, the laſt being not unfrequently weak. I had often

an opportunity of obſerving the pleaſant phenomenon, that the negative electricity ariſing from cold, after pulling off a cold and putting on a warm coat, was ſpeedily converted into poſitive, or the degrees next to it. When the thermometer ſtood ſeveral degrees above the freezing point, and a ſomewhat cold wind prevailed, the latter was ſufficient to produce negative or o electricity, as I experienced four times. Of 13 times, while I ſat lightly clothed in a warm apartment, the electricity was 8 times o, thrice —, and twice weakly +. Nay, when I clothed myſelf well, and remained any time at a cooler part of the room, ſuch as the open door or window, I found the electricity either negative or weakly poſitive. For producing this effect, it is in general ſufficient if a part only of the warm body be expoſed to cold. When the electricity was commonly +, while I ſat at perfect reſt, on my uſual chair, I oftentimes remarked that it was perceptibly negative if my feet were extremely cold, without being able to aſſign any other cauſe for this phenomenon. It is, however, fully proved by the following circumſtance: During 23 times that I waſhed my hands and face with cold water, the electricity was only 15 times perceptibly +, 8 times —, thrice o, and 7 times weakly +; though a moment before, or after, it was, for the moſt part, ſtrongly +.

That cold impedes poſitive electricity, or makes it leſs and reduces it to o, may be eaſily comprehended; becauſe cold cauſes every thing to contract, leſſens the pores of animal bodies, and, by theſe means, obſtructs the eſcape of the electric matter. That it, however, converts this electricity into negative, or gives to the external part of animal bodies the property of extracting from other bodies, when in contact with them, a part of their natural electricity, is not eaſy to be explained; for, in order to do ſo, they muſt themſelves have ſuſtained a loſs of their natural quantity of electricity. But when, by the contraction of the pores, they receive no more of the electric matter from the interior parts of the body, they muſt, in order that the equilibrium be kept up, not ſuſtain any loſs of it. This loſs is either real or apparent. The former can ſcarcely be aſſerted, as there are not ſufficient grounds for ſuch an idea. For if it be ſaid that the electric

matter naturally contained in the external part of the body, is expelled by that accumulated more and more in the interior parts, on account of the cold, and which thereby acquires a ſtronger repulſive power, it is of no weight; ſince this power would prevent the plate of the condenſer from giving up any of its natural electricity to the finger when applied to it. There is more reaſon, therefore, to aſſert the latter, *viz.* that this want is only apparent; that is, that the electric fluid, which the external parts of the body naturally poſſeſs, is not expelled by the effects of cold, but fixed, or ſo faſt united with the parts in which it is contained, that it loſes all movement, and gives no indications of its preſence. This kind of union is not uncommon in nature: it takes place in regard to the particles of fire, which produce heat; when moiſt bodies evaporate, or when ſalt is mixed with ice.

10. Laſſitude impedes alſo the poſitive animal electricity, or changes it into the oppoſite. During 16 times that I walked backwards and forwards in my apartment, or was otherwiſe employed, the temperature of the weather being 10, 12, 14, and more degrees of heat, and no cold wind prevailing at the time, I found the electricity only once weakly +, five times 0, and ten times —. In 32 experiments, when ſtanding at reſt, the electricity was 30 times 0, and twice weakly +. In 27 experiments, while I ſat at reſt, it was always ſtrongly +; and in five experiments, while walking at a moderate pace, it was perceptibly +. I found the caſe to be the ſame on my ſervant and another young man.

I will not, however, ſay that the electricity, when the body is in a ſtate of reſt, always ceaſes, or is changed from poſitive. Many times the ſtrength of the body is greater than to allow of its being exhauſted by fatigue, as I have more than once experienced.

11. By rubbing a part of the body, poſitive electricity is not impeded. The rubbing may be performed with linen or woollen.

12. Sudden, ſpeedy, and violent motion can change every kind of animal electricity into the oppoſite. A luminous and beautiful appearance was occaſioned when I inclined and raiſed my body ſuddenly in turns, and threw upwards quickly an

and with a certain degree of violence, ſometimes my arms and ſometimes my legs. Under theſe circumſtances the electricity was thrice changed from — to +, and 16 times from o, or weak +, to —. On my ſervant, under the ſame circumſtances, it was eight times changed from + to —; and on my maid five times from — to +.

13. Every other motion of the body, not connected with ſuch violence, or with uncommon agitation of the limbs, when it does not produce much fatigue, does not impede poſitive animal electricity.

14. Such motion is no impediment alſo to negative animal electricity.

15. After noon, or dinner-time, animal electricity is not greater than common.

16. The uſe of coffee makes no change in animal electricity.

17. Continual ſtraining of the thoughts is not only favourable to poſitive animal electricity, but increaſes it in an uncommon degree.

18. Animal electricity is impeded by perſpiration.

19. Animal electricity is ſtronger in winter than in ſummer.

20. Repoſe at noon, or any other ſhort ſleep when ſitting in the day-time, does not diſturb poſitive animal electricity.

21. The breath does not conduct animal electricity in a perceptible manner. I breathed on the cover of the condenſer in various places, but I never found the leaſt traces of electricity. The electricity, in all probability, was diſperſed too ſpeedily, or was too ſubtile to be perceptible.

22. Bodily motion is by no means neceſſary for producing animal electricity;

23. Neither does it depend on the movement occaſioned by reſpiration.

24. Animal electricity ariſes alſo without friction of the clothes. My experiments leave no doubt on this ſubject; as I found the electricity on my own body lively and durable for half an hour, or an hour, when I had on no clothes. I do not ſay, however, that the friction of the clothes does not increaſe it.

25. Animal electricity is excited without any friction of the external parts of the body.

26. There is alſo a ſpontaneous kind of animal electricity,

VI. *Obſervations on Pot-aſh; being an Inquiry how far the miſchievous Effects of Septic Acid are reſtrained by Pot-aſh and other Alkalies, particularly in reſpect to the Effects of Septite of Pot-aſh (Nitre or Saltpetre) upon Animal Fleſh intended to be eaten, and upon the Human Stomach; in a Letter from Dr. Mitchill to Dr. Prieſtley, dated Flandome, May 4, 1799. Communicated by Dr.* MITCHILL.

ON a former occaſion (2 Medical Repoſitory, p. 236 *et ſeq.*) an attempt was made to ſhew that the *ſeptic* acid, which is formed in certain putrefactive proceſſes, was materially different in its conſtitution and qualities from the *nitrous* acid obtained by diſtillation from ſaltpetre. Since the compoſition of that piece, I have obſerved that Juncker, in his view of the doctrines of Beecher and Stahl, (2 Conſpectus Chimiæ, p. 280,) is of the ſame opinion, declaring, that in whatever manner the work of ſeparating it from the putreſcent or other bodies with which it was naturally mingled, was undertaken, "ne micula tamen acidi nitroſi purè ſiſtitur," *not a particle of pure nitrous acid can be obtained.* And he warns his reader, that in the obſervations which he offers he means that *ſpirit of nitre* ONLY which is liberated from its connection with an alkaline ſalt. On the native ſeptic acid, which is, as he allows, furniſhed ſo largely by the animal kingdom, (p. 277,) and is by far the moſt active and intereſting form of oxygenated ſepton, Juncker, like moſt other writers, has ſaid ſcarcely any thing at all.

The highly deſtructive effects of this offspring of putrefaction have been pointed out in detail in a former eſſay (1 Medical Repoſitory, p. 39, 40), in which it was obſerved that certain ſubſtances, and among others pot-aſh in particular, poſſeſſed a power to reſtrain and curb its ferocity. The neutral ſalt, formed by the union of this peſtilential acid with the fixed alkali, is the ſaltpetre or nitre of the ſhops and of commerce. Concerning the medicinal and economical qualities of this ſubſtance, it is manifeſt to me that there are many miſtakes yet prevalent: and they appear to be worthy of being pointed out and corrected.

The

The ſeptite of pot-aſh has been denominated a ſalt of many excellent qualities, a *ſal polychreſtus*; and a great phyſician once wiſhed, for the good of his profeſſion, there could be found *one* other remedy ſo certain and ſteady in curing diſeaſes as nitre. It has likewiſe been termed an *antiphlogiſtic* remedy, good for all manner of inflammatory diſeaſes with phlogiſtic denſity of the blood; poſſeſſing fine attenuating powers, being in nowiſe acrimonious; and happily calculated to withſtand a putreſcent ſtate of the body. It has further been called a *refrigerant*, a *diuretic*, and a *carminative*, and employed accordingly by thoſe preſcribers who are influenced by the *nominal* efficacy of remedies.

Such are ſome of the ſuperlative effects aſcribed to this compound of the acid of peſtilence and pot-aſh: and for a conſiderable time after I became acquainted with the miſchievous effects, wrought occaſionally by the naked ſeptic acid, I remained in the belief that the ſtrongeſt of the alkalies could hold it faſt, and keep it entirely harmleſs. It therefore did not appear to me improbable that the character of the compound of the two might, as in a multitude of other caſes, be exceedingly different from that of either the conſtituent acid or alkali.

But latterly I have been inclined to the opinion, that pot-aſh is capable of combination with oxygenated ſepton in different degrees; that is to ſay, ſepton, before combining with the alkali, may have been united to *more* or *leſs* of oxygen; and alſo ſepton, in any of its degrees of oxygenation, may be united with pot-aſh in different proportions: in other words, the acid may *vary in its ſtrength*, and likewiſe, on every degree of ſtrength, may be united to the alkali *in various proportion*. The *nitrum nitratum*, deſcribed by the older chemiſts, is an example of pot-aſh *ſuper*-ſaturated with nitric acid, and, ſtrange to tell! has been extolled for its advantageous operation in ardent fevers, accompanied with thirſt and with a dry and foul tongue. I have ſtrong reaſon to think that there is a diſproportion between the acid and alkali in other forms of nitre; as I have known litmus-paper to be turned repeatedly reddiſh by a watery ſolution of ſalt-petre, the reſidue of a quantity which was ſwallowed by

miſtake, and which nearly deprived a man of his life. A ſet of correct experiments is wanting to elucidate more completely this part of a very curious and highly important ſubject.

Be theſe things as they may, all experience ſhews that the connexion between ſeptic acid and pot-aſh is eaſy to diſſolve; at leaſt, a portion of oxygen ſeparates very readily from the nitre. The experiment of reddening blood by mixing powdered ſaltpetre with it, was known to Hoffman, and, I think, fairly evinces a partial decompoſition of the ſalt. It ſeems to have a ſimilar operation upon the reſidue of that fluid in the fleſh of ſlaughtered animals, and the reddening of the lean and fibrous part of meat is evidently owing to the oxygen attracted from the nitre.

But a heightening of colour is not the only effect which ſeptite of pot-aſh works upon proviſions ſprinkled with it. There are in many meats, eſpecially of old animals, and of thoſe which have been a long time ſalted, a *toughneſs* and *hardneſs* which render them difficult to be cut and to be chewed. The ſeptic acid ſeems in ſome degree to be diſjoined from the pot-aſh, and evidently aſſiſts in decompoſing, to a certain point, the vaſcular and fibrous ſtructure of the meat. The quantity of nitre generally put on is ſmall; was the proportion larger, the meat would be yet further diſorganiſed, and be rendered more *ſhort* and *tender*, almoſt even to *rottenneſs*. But the injurious effect of the nitre is prevented by the ſea-ſalt commonly mingled with it, in the manner and upon the principle deſcribed in my "Obſervations on Soda." (2 Medical Repoſitory, p. 292, *et ſeq.*) The uſe of ſaltpetre, then, in curing proviſions, is to make them *reddiſh* and *tender*, and not to exerciſe an *antiſeptic* and *hardening* power, as the muriate of ſoda does.

With all theſe conſiderations before me, I entertained great doubts of the truth of thoſe fine things told over and again, and copied by one writer from another, about the mildneſs and wholeſomeneſs of nitre, and of its wonderful effects as a calmer and ſoother of diſeaſes. It ſeemed very ſtrange to me, that the acid which before its union with pot-aſh was capable of cauſing fevers, ſhould be ſo ſuddenly

transformed,

tranſformed, as, notwithſtanding the laxity of their coheſion, to cure ſuch diſeaſes immediately afterwards.

While I was conſidering theſe things, a caſe fell under my obſervation, which allowed me fully to witneſs the operation of this boaſted *cooler*, *carminative*, and *febrifuge*. It ſhews, beyond a doubt, that ſeptic acid, though coerced by pot-aſh, is in ſome degree ſeptic acid ſtill. Its native virulence does not even then wholly forſake it. Nitre ought to be ranked among the poiſons; for, in a ſufficient doſe, it is truly a poiſon. Though it may be adminiſtered in ſmall quantities without exciting terrible ſymptoms, it reſembles in that reſpect moſt other venomous ſubſtances, which can be ſwallowed without detriment, and even often with advantage, when their doſe is not too large. It is time for writers of Diſpenſatories, and on Materia Medica, to know the facts concerning nitre and its operation, and to publiſh them for the ſake of undeceiving their readers. Too long has the medical world been the dupe of idle and partial opinions on this ſubject. Read the following accident, and judge of what I have ſaid from the ſymptoms induced by ſwallowing ſeptite of pot-aſh or ſaltpetre:—

A carman, of middle age, had followed his buſineſs as uſual on Saturday the 20th day of April 1799. On Sunday morning he determined to take a ſaline cathartic for an indiſpoſition too trifling to require the advice of a phyſician. Accordingly his wife diſſolved an ounce of what ſhe believed to be ſulphate of ſoda (Glauber's ſalt) in water, and gave it to him at a draught. He ſwallowed it. Soon after he was ſeverely incommoded by what he termed "a great weakneſs about his heart." Nauſea came on, and was followed by vomiting of the contents of the ſtomach, mixed with conſiderable quantities of blood at each time. His ſtrength was exceedingly impaired: and a ſenſation of coldneſs over the whole body was remarked by him, particularly in the extremities. I did not ſee him until after the bloody vomiting had continued at times for ſeveral hours, and then his pulſes at the wriſt were very ſlow. He was however quite rational, and ſaid he then felt much better than he had done. On examining what kind of ſalt he had taken, for ſome cryſtals of it remained in a wide-mouthed bottle, I found it to be ſeptite

tite of pot-aſh (ſaltpetre.) This was about eleven o'clock before noon, and he had taken it at ſix in the morning. As it had had no purgative effect, I ordered him ſome caſtor-oil, and almond milk ſweetened with ſugar, or ſome milk-whey, and ſome water-gruel.

In 35 Commentar. de Rebus, &c. p. 176, a caſe is mentioned of death from taking an ounce of nitre: and if a part of the quantity which this man took had not been vomited up, he probably would have died too. The ol. ricini purged him gently, and he gradually got better, but complained very much of weakneſs about the præcordia.

There are numerous other accidents not materially unlike theſe. Such occurrences give us no very favourable account of the benignity of nitre as a medicine. It is a pity that practiſers of phyſic do not better underſtand the component parts of their preſcriptions. How few know that, in adminiſtering nitre, their patients are made to ſwallow a portion of the naufeating and ſickening acid of putrefaction!

Poſſibly theſe remarks may have a tendency to remove the doubt contained in your letter of April 11, 1799. You will hereby perceive that *my native acid of ſepton* is a combination of this baſis with oxygen and water; whereas your *artificial acid of nitre* undergoes a partial decompoſition by the heat of diſtillation, and is adulterated beſides with whatever happens to be mingled with it during and after its combination with the vegetable fixed alkali. And both theſe forms of acid differ from atmoſpherical air; inaſmuch as the former are *chemical* mixtures, the latter is *mechanical*.

It would be better for ſcience if the word "nitre" was rejected altogether from uſe. *Nitria*, whence the term comes, was, you know, a diſtrict of ancient Egypt, famous for the quantity of mineral alkali which it afforded. (D'Anville's Geograph.—Egypt.) This ſaline ſubſtance has thence been called by the names *nitrum*, *nitre*, &c. In confirmation of which, I obſerve, in the Dictionary of Calepinus, printed at Baſil in 1538, that what they called *nitrum* was a material employed to cleanſe clothes, and waſh the bodies which wore them. And S. Bochart remarks, (1 Opera. Chanan, L. II. cap. xiv.) that the ancients made a kind of ley from *aſhes*, *ſoda*,

ſoda, and *bole*, (*cinere*, *nitro*, et *cimoliâ*,) for more effectually clearing their bodies from naſtineſs when they bathed. I need not remark to you, that I employ the word "nitre," not in its ancient, but modern ſenſe.

It is highly deſirable that ſome of our men, whoſe opinions have weight with the public, would peruſe the work of Lanciſi, phyſician to Pope Clement XI, on the noxious exhalations of marſhes. (De Nox. Palud. Effluviis.) By the perſevering and luminous reſearches of this great man, it was found, as long ago as the year 1716, that marſh-water, by ſimple diſtillation, (*Ibid.* Lib. II. c. xii.) manifeſted an *acid* quality; and that *calcareous* ſtones (*Ibid.* Lib. II. c. ii.) were better for paving the ſtreets of cities than *ſiliceous* ones, becauſe the *alkaline nature* of the former was adapted to imbibe the noxious moiſture of the air, and ſweeten the *acid ſalts* with which it abounded. Indeed, much of the matter detailed by the writers of our days on local ſources of diſtempers, may be found better obſerved, and better ſtated, by Lanciſi, than in their writings. By the by I obſerve he mentions the Engliſh philoſopher Mayow, (*Ibid.* Lib. II. P. ii. cap. 2.)

If the philoſopher of Rome had reaſoned upon his own diſcovery, he could not have failed to draw the inference, that by *alkaline ſubſtances* might the HYDRA of peſtilence be overcome.—As I have mentioned this monſter, I ſhall pauſe a little to give you my opinion of the allegory among the ancients concerning her: and I attempt the explanation the more willingly, as I believe Lord Verulam has ſaid nothing about it. The fable is this:—In Peloponneſus, between Mycenæ and Argos, there was a fen or marſh of ſome extent called Lerna. This muddy and ſtagnating pool was inhabited by Hydra, a horrible and devouring monſter with ſeveral heads; ſome ſay ſeven, others nine, and others fifty. The malignity of her poiſon was ſuch that a wound from an arrow dipped into it was inſtantly mortal. She made dreadful havoc among the people of the ſurrounding country, and devoured a great number of their ſheep and other cattle. In obedience to the orders of the tyrant Euryſtheus, Hercules went to fight this deſtructive and formidable creature. On his

his approach, a crab came forth to the aſſiſtance of Hydra: but Hercules cruſhed the crab, and afterwards ſlew Hydra. Of the heads of Hydra, it was reported, when one was cut off, two would ſprout from the wound, unleſs prevented by the immediate application of fire. Hercules, availing himſelf of the aid of fire, ſucceeded in his undertaking. In the ninth figure of Montfaucon's 66th plate, there is a figure of Hercules with crabs near his feet, having, as the learned father curiouſly enough remarks, a relation to ſome myſtery which he does not comprehend. (1 Antiquity explained, Art. Hercules, chap. ix.)

Now, it appears to me, this is an allegory expreſſive of the peſtilential vapours emitted by the bog of Lerna, and of the means found by experience uſeful to drain off its ſtagnant water, and to clear the adjoining and ſurrounding moraſſes.

The word "hydra" is derived from ὑδωρ, *water*. This fluid then, detained upon the marſh of Lerna, favoured occaſionally the production of unwholeſome exhalations. Such vapours, being at once inviſible and injurious, were aſcribed to ſome preternatural enemy or deſtructive monſter; and being diffuſed, or wafted around the country, and oftentimes cutting off both man and beaſt, were fancied to be the effect of the ſuppoſed monſter's poiſon. According to their extent and virulence was ſhe reported to have fewer or more heads for preparing and inflicting this poiſon. The mere draining off the water, and leaving the mud and ſlime bare, was termed cutting off an head; and the increaſe of deleterious gaſes, in conſequence of expoſing ſuch a naked ſurface, was aptly expreſſed by the ſprouting forth of two in its place. By cauteriſing, or ſearing, was underſtood either the ſolar heat in drying the ground after the water was drained away, or the burning up of the trees, ſhrubs, and obſtacles to free ventilation by ordinary combuſtion, or perhaps both. The crab, who was Hydra's ally, perhaps does not refer to the ſun's place in the conſtellation Cancer, ſo much as to ſhew the frequent recurrence of the difficulties, and the ſuperior ſtrength and ſkill requiſite to overcome them. In the whole allegory "Hercules" may be underſtood to mean "inſuperable courage and induſtry." North America, at the cloſe of the 18th

century, wants a Hercules. This interpretation is confirmed by another conſideration, that the ancients had not only their *Hydra*, who lived in the water, but their *Cherſydra*, who remained after the marſh or fen was dried up. *Cherſydra*, being derived from the two words, χερσος, *land not fit for the plough*; and υδρα, *the monſter of the fens*; will thus mean the venomous and ſickly condition of the neighbouring atmoſphere after the water was exhaled, and the ground at the ſame time not rendered arable thereby, typified by a poiſonous ſerpent: and was thus expreſſive of the rage of peſtilential efflavia, which ſometimes, and under certain circumſtances, continue in a virulent ſtate, in dry weather, near their dried ſources.

Hydra is ſeemingly mentioned by Virgil (Æn. vi. v. 576.) as a *fictitious* or *poetical* animal. Bochart, however, with his uſual prodigious erudition, appears deſirous to make the whole ſtory *literally* intelligible. (Hierozoici pars poſter. Lib. III. cap. xiii.) But Lanciſi, with a more clear and diſcriminating mind, perceives that important phyſical truths are concealed under this two-fold allegory, and ſhews how they are to be unriddled. (De Nox. Palud. Effl. Lib. II. p. ii. cap. 3.)—In conſidering theſe matters you will not fail to recollect that the claſſical writers, and others, uſe the word "hydrus" as well as "hydra," and ſome of them apparently confound the two. The former noun of the maſculine gender is probably the name of the *real* animal, the *water-ſnake*, the latter of the *imaginary* one.—But of this enough.

If peſtilential matter, as I obſerved before, can be ſubdued by alkalies, then the formation of ſeptite of pot-aſh in the alimentary canal muſt be a very frequent and common proceſs. It is univerſally agreed that peſtilential matter may be taken into the ſtomach by ſwallowing; and no reaſonable doubt can be entertained of its production within that organ, and other parts of the inteſtinal tube, from the corrupted remains of food. The whole tenor of preſcription, as explained in my letter concerning the uſe of alkaline remedies in fevers, (1 Medical Repoſ. p. 265.) goes to ſhew that the offending cauſe is of an acid quality: and in its worſt forms, I think the acid engendered has ſepton for its baſis; ſince aliment,

containing this principle only, is capable of yielding that moſt noxious compound. When ſeptic acid thus exiſts in the alimentary canal, and carbonates, tartrites, &c. of pot-aſh, are taken down, the ſtronger acid is attracted, and a proportional quantity of nitre or ſaltpetre is formed. Thus, in abundance of caſes, the practiſers who are fond of preſcribing ſeptite of pot-aſh might ſpare themſelves the trouble, as that ſaline compound is, in certain caſes, formed in the ſtomach and bowels in conſiderable quantity. What preciſe effect the *nitre ſo formed* has on the inteſtines, and on the conſtitution at large, deſerves to be inquired into with great care; for I am not without apprehenſion that ſome of the ſad ſymptoms, occaſionally attendant on fevers, are modified by the ſeptite of pot-aſh formed within the belly.—I believe, however, I muſt now ſtop, and trouble you no further for the preſent than by aſſuring you, that I hope much from your aid in this inquiry, which you inform me you conſider "as a happy train of diſcovery;" and that I am, very reſpectfully, yours,

SAM. L. MITCHILL.

To Dr. Prieſtley.

VII. *Deſcription of a Mercurial Gazometer conſtructed by Mr.* W. H. Pepys *jun. Communicated by the Author.*

THE difficulty attending the exhibition of acid and alkaline gaſes, was the firſt inducement to Prieſtley, Lavoiſier, and others, to uſe mercury for ſuch experiments; but the great expence and enormous weight of this fluid obliged many accurate and experienced chemiſts to relinquiſh them almoſt entirely, as there appeared no other likely means of ſucceeding but by its means and that of the exhauſted receiver. A contrivance to leſſen the quantity of mercury neceſſary for ſuch experiments was therefore a deſirable object; and by introducing the dome uſed by Mr. Watt in his hydraulic bellows, I have ſucceeded in conſtructing an apparatus in which both of the above-mentioned requiſites are obtained at a comparatively ſmall expence. As it is hardly poſſible

poſſible to japan tin or copper ſo perfectly as to prevent their being attacked by mercury when brought in contact with them, I had the cylinders turned in lignum vitæ, on which the mercury has no action: the conducting tube is of glaſs; and the cocks are coated on the inſide with varniſh.

The uſefulneſs of an apparatus ſo conſtructed will appear ſufficiently obvious, when it is recollected that all gaſes paſſed through any other fluid than mercury, water for inſtance, take up a quantity of moiſture, which adds conſiderably to their gravity, and makes it impoſſible to determine their real weight.

For weighing of gaſes I make uſe of a glaſs globe and ſtop-cock of a ſmaller ſize than is commonly employed, as greater accuracy can be obtained by uſing a proportionally delicate beam than by employing a larger globe, which muſt be ſuſpended to a beam of ſuch ſtrength as greatly to increaſe the friction on its axes. It is of great importance, in the analyſis of bodies, or in other chemical experiments, to be able to aſcertain with accuracy the weight of any gas obtained by the proceſs. The weight of two or more quantities of gas ſhould however be always tried, and the mean be taken to prevent any error.

A, (Plate III.) is a repreſentation of the bell of the gazometer, made of glaſs, furniſhed with a cock at top, and able to contain 34 ounces troy of diſtilled water. The diviſions of capacity, determined by actual meaſurement, are marked on the glaſs with a diamond. BB, ſection of two cylinders of lignum vitæ, the outward one ſcrewed upon the ſolid internal one, which is made to project at its lower extremity, and furniſhed with a male ſcrew, to work into a female ſcrew with which the lower end of the external cylinder is furniſhed. The ſpace between theſe is ſo adjuſted as to be almoſt filled up by the ſubſtance of the glaſs bell A when dropped into it, ſo that the quantity of mercury neceſſary to fill up that ſpace is proportionally ſmall. The internal cylinder has a conducting tube up through its axis, the lower end of which is furniſhed with a female ſcrew anſwering to the male ſcrew of the cock of the ſmall receiver C. The receiver C is made of glaſs, and open at bottom. When this

receiver is used, it is screwed into its place, and rests upon a small cup or cistern of mercury D, in which the beak of a retort, furnished with a bent glass tube, to be afterwards noticed, may be introduced under the receiver. E,E,E,E, section of a wooden stand upon which the cylinders of lignum vitæ are supported, having an opening through the top to permit the cock of the receiver C to be joined to the conducting tube of the internal cylinder B. The cistern D is adjusted to its height by means of a rising cylinder in the pedestal F. G is a transfer glass for mixing alkaline gases in vacuo, or other purposes; and, when used, is joined to the top of the bell A. H, a glass globe and stop-cock, capable of holding 14 ounces troy of distilled water for weighing gases: it receives its gas by being inverted, and screwed into the bell A. I, a bladder furnished with a stop-cock to assist in holding, transferring, or mixing different gases. K, an elastic gum-bottle, capable of containing 30 ounces of distilled water, for holding the acid gases: when used, it is screwed into the top of the transfer G; the bottom cock of the latter being at the same time joined to the bell A, previously charged with the alkaline gas: the cocks being turned, the gases rush together in vacuo. L, a small portable air-pump, for exhausting the globe H. M, a double male screw, which fits any part of the apparatus, and on which a valve may be fastened. N, a double female screw. O, a small instrument, which I would not have mentioned, had I not found it of peculiar service in collecting spilled mercury: it is made of glass, the mouth being applied at *a*: you may collect any small globules of mercury by the small end *b*, by which means they are elevated into the receiver at O. It is useful likewise for removing mercury from the small cistern.

One of the principal objections to the use of mercury in such experiments as this apparatus is intended for, has been, the great force necessary to overcome the resistance of a column of mercury when gases are to be received over that dense fluid; a resistance in the proportion of one inch of mercury to fourteen inches of water, and which very few lutes are able to withstand. This resistance I overcome by a very simple contrivance: a bent tube fitted into the beak

of

of the retort, (if one be employed,) or into a Wolf's apparatus, and paſſing into the upper part of the ſmall receiver, as expreſſed in the plate at C. By employing mercury for ſuch experiments, another advantage is gained by the uſe of this apparatus, namely, a power of exhauſtion in the retort, or Wolf's bottle, equal to a column of two inches of mercury, or 28 inches of water. This will be eaſily conceived when it is recollected that, by drawing up the large receiver A, the ſmall one C is raiſed in its ciſtern, bearing up with it the contained mercury, which is kept in its place by the preſſure of the atmoſphere on the ſurface of the mercury in the ciſtern. The cock of the ſmall receiver C is then to be turned off, and that of the large one A to be turned on. The air, of which the retort, or Wolf's bottle, is thus exhauſted, may then be let out, by plunging A into the mercury between the cylinders BB, and turning off the cock. When a ſufficient quantity of gas paſſes from the retort, or bottle, through the bent tube into C, to level the mercury in it and the ciſtern, the communication may again be opened, and the ſame ſteps followed as before deſcribed. By this means I have been enabled to obtain more gas, from the ſame materials, than if I had received it through a fluid of the weight of water; a circumſtance of ſome importance where nice and accurate reſults are looked for.

The plate of the apparatus is on a ſcale of nearly three inches to a foot.

VIII. *A Communication reſpecting the Preparation of Writing Ink. By Mr.* Desormeaux *junior.*

Mr. Editor,

BEING a conſtant reader of your truly inſtructive and entertaining Work, and having noticed in the Number for October, p. 29, a communication relative to an improved writing ink, I am, in conſequence, induced to offer ſome practical remarks on that ſubject.

Previous to my reading that communication reſpecting the diſcoveries of Prouſt having been applied by Van Mons

to

to the preparation of the fluid alluded to, I had made a great variety of experiments, in ſome of which I treated the ſulphat of iron preciſely as there directed; but ſince that time, I have, with a view to improvement, followed the rule which is there preſcribed ſtrictly, and have beſides, at former intervals, gone over and varied the experiments of Macquer, Lewis, Berthollet, Ribancourt, Prouſt, and other celebrated chemiſts, both foreign and native, who have treated on this and other ſubjects nearly allied to it; with a view, if poſſible, to eſtabliſh certain data upon which to ground the beſt mode of fabricating the article in queſtion, being very deſirous conſtantly to obtain an ink for the common purpoſes of penmanſhip, which, at the ſame time that it ſhould flow with freedom, ſhould have the proper degree of luſtre or gloſſineſs, and be at the moment of uſing intenſely black, with a capability of retaining that colour continually, even though openly expoſed to the action of the ſun and air. This, I preſume, I have accompliſhed: at leaſt, during a ſpace of ten years, I have never met with any formula which has ſo well rewarded my pains as the one which I am about to communicate. Having been brought up in a line of buſineſs in which I am in the daily habit of obſerving the action of ſuch ſubſtances upon each other as enter into the formation of ink, it may be readily imagined that, in a practical point of view, my opportunities for the improvement of it have fallen little ſhort of any individual whatever. If to this I add, that, ſince the year 1794, I have annually ſupplied the public with at leaſt 17,000 gallons of ink, beſides preparing powder for its occaſional production, it may be inferred that I have ſpared no pains in ſtudying what would beſt conduce to its perfectability; and after the many attempts which I have made, I have never found my expectation or that of my friends deceived, when the proceſs has been conducted agreeably to the following directions:—In ſix quarts (beer meaſure) of water, (it does not appear of importance whether it be rain, river, or ſpring water,) *boil* four ounces of the beſt Campeachy logwood, chipped very thin acroſs the grain; (the boiling may be continued near an hour;) adding from time to time a little *boiling* water, to compenſate for waſte by evaporation: ſtrain

the liquor *whilſt hot*, ſuffer it to cool, and make up the quantity equal to five quarts by the further addition of cold water. To this *cold* decoction put one pound averdupois weight of blue galls, or 20 ozs. of the beſt galls in ſorts, which ſhould be firſt coarſely bruiſed; 4 ozs. of ſulphat of iron, calcined to whiteneſs; ½ oz. of the acetite of copper, which ſhould be triturated in a mortar, moiſtened by a little of the decoction gradually added till it be brought to the form of a ſmooth paſte, and then thoroughly intermixed with the whole maſs. Three ounces of coarſe brown ſugar, and ſix ounces of good gum Senegal, or Arabic, are alſo to be added. Theſe ſeveral ingredients may be introduced one after the other immediately, contrary to the advice of ſome, who recommend the gum, &c. to be added when the ink is nearly made; as gum, however, is at preſent exorbitantly dear, three or four ounces will be found ſufficient, with only one and an half ounce of ſugar, unleſs, for particular purpoſes, it is wanted to bear a higher gloſs than common. In regard to the uſe of ſugar, which I have here recommended, I hope I ſhall not treſpaſs in remarking, that my obſervations, on a very extenſive ſcale, are coincident with thoſe of M. Ribancourt, who ſays, that a degree of fluidity is by its means imparted, which allows the doſe of gum to be enlarged conſiderably beyond what it would bear without it; and it is thereby rendered leſs liable to clog the pen, which, eſpecially when the nib is very fine, if it does not flow freely, quits it ſo ſlow as unpleaſantly to retard the writer, and is totally unfit for fine manuſcripts; beſides, by ſuch an union of gum and ſugar, a degree of conſiſtency is given to the liquor, which enables it to ſuſpend a much greater portion of colouring matter than otherwiſe could be effected; a circumſtance of the greateſt importance to its hue and permanency. It is far from my deſign, in thus tranſmitting my ideas, to induce a ſuppoſition that it is *exactly* after this manner that I, or any other perſon who ſupply ink by wholeſale, compound it; ſo far from it, that the firſt coſt of one gallon, on the plan here ſuggeſted, would exceed twice the ſum which is uſually paid for it to the manufacturer, although by retail it is diſpoſed of at an enormous rate. For private perſons, therefore, who wiſh to

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be particular in their manuſcripts, the rule I have here laid down will be found fully to anſwer their expectations at a cheaper rate than they are in general furniſhed with it. The beſt veſſel, in my opinion, in which to make ink, is a common ſtone bottle, which will contain half as much more as is propoſed to be made, and which ſhould be agitated twice a day, and be left uncorked, in order to expoſe freſh ſurfaces of the liquor to the action of the air, without which it cannot be expected to write very black when firſt uſed, but with this precaution may be fit for uſe in about 14 days; when it may be poured from its dregs, and corked up, to preſerve it from duſt, as well as waſte by evaporation. In cold weather it ſhould never be ſuffered to freeze, which is found to cauſe a diſunion of its parts, nor does it ever afterwards recover its former intenſity of colour, luſtre, or durability. Many perſons, I am aware, diſapprove, and therefore omit the uſe of logwood, from an apprehenſion that it induces a tendency in the ink to fade; this however, from the ſtricteſt attention, I can aſſert to be fallacious, at leaſt where it is not had recourſe to by way of ſubſtitute for the other more expenſive articles; indeed its effects are quite the reverſe, where all the ingredients are properly proportioned. As to the beautiful complexion which is given by it to the entire maſs of fluid, it would be needleſs to inſiſt, unleſs for the ſake of thoſe who are unacquainted how a mixture of the acetite of copper and logwood liquor work together, and by which, in this inſtance, a richneſs and bloom is given, which can in no other way be got with equal economy and ſucceſs; independent of which, the colouring matter of the wood, by its affinity with the oxyde of iron, has a very powerful effect to blacken the ink, and to render it leſs capable of change from any unſaturated acid in the ſulphat of iron, or from the operation of the air. I am aware, too, that the introduction of cupreous matter has been objected to, on the ſcore of its injuring the penknife by a portion of copper attaching itſelf thereto in conſequence of ſuperior elective attraction, whereby it has been remarked that a part of the knife's edge is diſſolved, and a quantity of copper anſwering thereto is exchanged and depoſited in its place, which is always ſaid to happen when the pen retains a little

a little ink at the time of its being mended. This, I think, is ſpinning out the effect of chemical affinity too far; for although, with reſpect to the reaſoning, the truth muſt be admitted, I contend, notwithſtanding, that its conſequences to the edge are not apparent to the eye; nor is any ſenſible detrimental change thereby produced upon the knife, more than what is induced by any other ink into which neither the ſulphat or the acetite of copper is introduced. As to myſelf, however, I have never yet ſeen a receipt for the formation of ink, where each of the principles were ſo completely proportioned and ſaturated with each other as to be inert when applied to the ſurface of a piece of poliſhed metal, ſuch as iron, for example; nay, even the frequent contact of water only, if allowed to remain, would ſpoil any inſtrument made of that material. I ſhall not enlarge on this particular, conceiving the moſt powerful objection I can offer in oppoſition to that opinion, is, that I have now lying by me a penknife, which has been in conſtant uſe between two and three years, and which even yet retains almoſt as good an edge as it had at firſt, by the mere aſſiſtance of an unprepared leather ſtrap, although in the ink with which I write the acetite of copper has always been uſed. But even admitting, for argument's ſake, that what I have attempted to diſprove is true, how many hundred pens might be made and mended ere it would be requiſite to beſtow twopence to the cutler for the exerciſe of his art? In ſhort, if the introduction of the ſubſtance alluded to into ink be a fault, I conſider it as one of no conſequence when put in competition with the advantages of beauty, durability, and intenſity of colour imparted by it.

I ſhall intrude no longer on the time or patience of your readers, than by requeſting, if any of them are poſſeſſed of a cheap and efficacious method whereby the mouldineſs of ink may be prevented, that they will communicate the ſame through the medium of your Magazine, in doing which a very conſiderable ſervice will be rendered towards its perfectability. The admixture either of a ſmall quantity of the muriate of ſoda, of the nitrate of pot-aſh, of alcohol, or of cloves, have been ſeverally recommended for this purpoſe; but by experience I know that of theſe only alcohol will

avail, and this cannot be added in ſufficient quantity without cauſing it to ſink into, and ſpread upon the paper; ſo that, indeed, *its* uſe is altogether interdicted. The beſt plan which I have hitherto found as a preventive to the vegetative proceſs I here allude to, is, to add the ingredients of which the ink is compoſed to the *cold* decoction; if recourſe be had to boiling, ALL *together*, it is found very rapidly to promote the inconvenience; and were ebullition totally avoided, it would on that account be better, but then we could not obtain the ink of ſo deep a colour; yet I know of no inſtance in which mouldineſs will not appear in time, and that in no inconſiderable degree. In fine, I am ignorant of any ſubſtance, or method, that has been hitherto uſed as a ſpecific to remedy the defect. If there is any that can be ſuggeſted, I ſhould be happy to put it to the teſt of experiment.

I am, Sir, yours, &c.

L. DESORMEAUX jun.

No. 8, Vine Court, Spitalfields.

IX. *An Inquiry concerning the Weight aſcribed to Heat. By* BENJAMIN COUNT RUMFORD, *F.R.S. M.R.I.A.* &c, *Read before the Royal Society May* 2, 1799.

THE various experiments which have hitherto been made with a view to determine the queſtion ſo long agitated, relative to the weight which has been ſuppoſed to be gained, or to be loſt, by bodies upon their being heated, are of a nature ſo very delicate, and are liable to ſo many errors, not only on account of the imperfections of the inſtruments made uſe of, but alſo of thoſe, much more difficult to appreciate, ariſing from the vertical currents in the atmoſphere, cauſed by the hot or the cold body which is placed in the balance, that it is not at all ſurpriſing that opinions have been ſo much divided relative to a fact ſo very difficult to aſcertain.

It is a conſiderable time ſince I firſt began to meditate on this ſubject, and I have made many experiments with a view to its inveſtigation; and in theſe experiments I have taken all

all thoſe precautions to avoid errors, which a knowledge of the various ſources of them, and an earneſt deſire to determine a fact which I conceived to be of importance to be known, could inſpire: but though all my reſearches tended to convince me, more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been ſo ſenſible of the delicacy of the inquiry that I was for a long time afraid to form a decided opinion upon the ſubject.

Being much ſtruck with the experiments recorded in the Tranſactions of the Royal Society, Vol. LXXV. made by Dr. Fordyce, upon the weight ſaid to be acquired by water upon being frozen; and being poſſeſſed of an excellent balance, belonging to his moſt Serene Highneſs the Elector Palatine Duke of Bavaria, early in the beginning of the winter of the year 1787 (as ſoon as the cold was ſufficiently intenſe for my purpoſe) I ſet about to repeat thoſe experiments, in order to convince myſelf whether the very extraordinary fact related might be depended on; and with a view to removing, as far as was in my power, every ſource of error and deception, I proceeded in the following manner:—

Having provided a number of glaſs bottles, of the form and ſize of what in England is called a Florence flaſk (blown as thin as poſſible) and of the ſame ſhape and dimenſions, I choſe out from amongſt them two, which, after uſing every method I could imagine of comparing them together, appeared to be ſo much alike as hardly to be diſtinguiſhed.

Into one of theſe bottles, which I ſhall call A, I put 4107,86 grains troy of pure diſtilled water, which filled it about half full; and into the other, B, I put an equal weight of weak ſpirit of wine; and, ſealing both the bottles hermetically, and waſhing them and wiping them perfectly clean and dry on the outſide, I ſuſpended them to the arms of the balance, and placed the balance in a large room, which for ſome weeks had been regularly heated every day by a German ſtove, and in which the air was kept up to the temperature of 61° of Fahrenheit's thermometer, with very little variation. Having ſuffered the bottles, with their contents, to remain in this ſituation till I conceived they

muſt have acquired the temperature of the circumambient air, I wiped them afreſh with a very clean dry cambric handkerchief, and brought them into the moſt exact equilibrium poſſible, by attaching a ſmall piece of very fine ſilver wire to the arm of the balance, to which the bottle which was the lighteſt was ſuſpended.

Having ſuffered the apparatus to remain in this ſituation about twelve hours longer, and finding no alteration in the relative weights of the bottles, (they continuing all this time to be in the moſt perfect equilibrium,) I now removed them into a large uninhabited room fronting the north, in which the air, which was very quiet, was at the temperature of 29° F., the air without doors being at the ſame time at 27°; and, going out of the room, and locking the door after me, I ſuffered the bottles to remain forty-eight hours, undiſturbed, in this cold ſituation, attached to the arms of the balance as before.

At the expiration of that time I entered the room, uſing the utmoſt caution not to diſturb the balance, when, to my great ſurpriſe, I found that the bottle A very ſenſibly preponderated.

The water which this bottle contained was completely frozen into one ſolid body of ice; but the ſpirit of wine, in the bottle B, ſhewed no ſigns of freezing.

I now very cautiouſly reſtored the equilibrium, by adding ſmall pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was ſuſpended, when I found that the bottle A had augmented its weight by $\frac{1}{3\cdot5\frac{1}{9}04}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains troy, (the bottle weighing 703,37 grains, and the water 4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the oppoſite arm of the balance, to counterbalance it.

Having had occaſion juſt at this time to write to my friend, Sir Charles Blagden, upon another ſubject, I added a poſtſcript to my letter, giving him a ſhort account of this experiment, and telling him how "*very contrary to my expectation*" the reſult of it had turned out: but I ſoon after found that I had

had been too haſty in my communication. Sir Charles, in his anſwer to my letter, expreſſed doubts reſpecting the fact; but, before his letter had reached me, I had learned, from my own experience, how very dangerous it is, in philoſophical inveſtigations, to draw concluſions from ſingle experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which ſtill remained at the temperature of 61°,) the ice in the bottle A gradually thawed; and being at length totally reduced to water, and this water having acquired the temperature of the ſurrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment before the water was frozen.

This experiment, being repeated, gave nearly the ſame reſult, the water appearing, when frozen, to be heavier than in its fluid ſtate; but ſome irregularity in the manner in which the water loſt the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as alſo a ſenſible difference in the quantities of weight apparently acquired in the different experiments, led me to ſuſpect that the experiment could not be depended on for deciding the fact in queſtion: I therefore ſet about to repeat it, with ſome variations and improvements:—but, before I give an account of my further inveſtigations relative to this ſubject, it may not be amiſs to mention the method I purſued for diſcovering whether the appearances mentioned in the foregoing experiments might not ariſe from the imperfections of my balance; and it may likewiſe be proper to give an account, in this place, of an intermediate experiment which I made, with a view to diſcover, by a ſhorter route, and in a manner leſs exceptionable than that above-mentioned, whether bodies actually loſe, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My ſuſpicions reſpecting the accuracy of the balance aroſe from a knowledge (which I acquired from the maker of it) of the manner in which it was conſtructed.

The three principal points of the balance having been determined, as nearly as poſſible, by meaſurement, the axes of

motion were firmly fixed in their places in a right line, and the beam being afterwards finiſhed, and its two arms brought to be in equilibrio, the balance was proved by ſuſpending weights, which before were known to be exactly equal, to the ends of its arms.

If with theſe weights the balance remained in equilibrio, it was conſidered as a proof that the beam was juſt; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, ſuſpended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though in this method of conſtructing balances the moſt perfect equality in the lengths of the arms may be obtained, and conſequently the greateſt poſſible accuracy, when uſed at a time when the temperature of the air is the ſame as when the balance was made; yet, as it may happen that, in order to bring the arms of the balance to be of the ſame length, one of them may be much more hammered than the other, I ſuſpected it might be poſſible that the texture of the metal forming the two arms might be rendered ſo far different by this operation as to occaſion a difference in their expanſions with heat; and that this difference might occaſion a ſenſible error in the balance, when, being charged with a great weight, it ſhould be expoſed to a conſiderable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, aroſe in any degree from this cauſe, I had only to repeat the experiment, cauſing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a ſenſible difference in the reſults of different repetitions of the ſame experiment, made as nearly as poſſible under the ſame circumſtances, and as it was above all things of importance to aſcertain the accuracy of my balance, I preferred making a particular experiment for that purpoſe.

My firſt idea was, to ſuſpend to the arms of the balance, by very

very fine wires, two equal globes of glaſs filled with mercury, and, ſuffering them to remain in my room till they ſhould have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have ſeen if they ſtill remained in equilibrio under ſuch difference of temperature: but, conſidering the obſtinacy with which moiſture adheres to the ſurface of glaſs, and being afraid that ſomehow or other, notwithſtanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increaſed; and having found, by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt ſurfaces of metals do not attract moiſture; inſtead of the glaſs globes filled with mercury, I made uſe of two equal ſolid globes of braſs, well gilt and burniſhed, which I ſuſpended to the arms of the balance by fine gold wires.

Theſe globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61) of my room, in which they were expoſed more than twenty-four hours, were brought into the moſt ſcrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The reſult of this trial furniſhed the moſt ſatisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts reſpecting the accuracy of my balance, I now reſumed my inveſtigations relative to the augmentation of weight which fluids have been ſaid to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as poſſible againſt every ſource of error and deception. The bottles being of the ſame ſize, neither any occaſional alteration in the preſſure of the atmoſphere during the experiment, nor the neceſſary and unavoidable difference in the denſities of the air in the hot and in the cold rooms in which they were weighed, could affect

their

their apparent weights; and their ſhapes and their quantities of ſurface being the ſame, and as they remained for ſuch a conſiderable length of time in the heat and cold to which they were expoſed, I flattered myſelf that the quantities of moiſture remaining attached to their ſurfaces could not be ſo different as ſenſibly to effect the reſults of the experiments. But, in regard to this laſt circumſtance, I afterwards found reaſon to conclude that my opinion was erroneous.

Admitting the fact ſtated by Dr. Fordyce, (and which my experiments had hitherto rather tended to corroborate than to contradict,) I could not conceive any other cauſe for the augmentation of the apparent weight of water, upon its being frozen, than the loſs of ſo great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loſs of latent heat added to the weight of one body, it muſt of neceſſity produce the ſame effect on another, and conſequently, that the augmentation of the quantity of latent heat muſt, in all bodies, and in all caſes, diminiſh their apparent weights.

To determine whether this is actually the caſe or not, I made the following experiment:—

Having provided two bottles, as nearly alike as poſſible, and in all reſpects ſimilar to thoſe made uſe of in the experiments above-mentioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and ſealing them hermetically, and ſuſpending them to the arms of the balance, I ſuffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained twenty-four hours. But there was not the leaſt appearance of either of them acquiring or loſing any weight.

Here it is very certain that the quantity of heat loſt by the water muſt have been very conſiderably greater than that loſt by the mercury, the ſpecific quantities of latent heat in water and in mercury having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat loſt, produced no ſenſible difference on the weights of the fluids in queſtion.

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Had any difference of weight really exiſted, had it been no more than *one millionth* part of the weight of either of the fluids, I ſhould certainly have diſcovered it; and, had it amounted to ſo much as $\frac{1}{7000}$ part of that weight, I ſhould have been able to have meaſured it: ſo ſenſible, and ſo very accurate, is the balance which I uſed in theſe experiments.

I was now much confirmed in my ſuſpicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, aroſe from ſome accidental cauſe; but I was not able to conceive what that cauſe could poſſibly be,—unleſs it were either a greater quantity of moiſture attached to the external ſurface of the bottle which contained the water, than to the ſurface of that containing the ſpirits of wine,—or ſome vertical current or currents of air, cauſed by the bottles, or one of them, not being exactly of the temperature of the ſurrounding atmoſphere.

Though I had foreſeen, and, as I thought, guarded ſufficiently againſt theſe accidents, by making uſe of bottles of the ſame ſize and form, and which were blown of the ſame kind of glaſs, and at the ſame time, and by ſuffering the bottles in the experiments to remain for ſo conſiderable a length of time expoſed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice, and of ſpirit of wine, with reſpect to heat; or, in other words, the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are expoſed, or the time taken up in that operation; and, conſequently, was not *abſolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with ſuch variations as ſhould put the matter in queſtion out of all doubt.

I was the more anxious to aſſure myſelf of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by cauſing unequal currents in the air, but alſo by cauſing, at the ſame time, a greater or leſs quantity of moiſture to remain attached to the glaſs.

To remedy theſe evils, and alſo to render the experiment more ſtriking and ſatisfactory in other reſpects, I proceeded in the following manner:—

Having provided three bottles, A, B, and C, as nearly alike as poſſible, and reſembling in all reſpects thoſe already deſcribed; into the firſt, A, I put 4214,28 grains of water, and a ſmall thermometer, made on purpoſe for the experiment, and ſuſpended in the bottle in ſuch a manner that its bulb remained in the middle of the maſs of water; into the ſecond bottle, B, I put a like weight of ſpirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

Theſe bottles, being all hermetically ſealed, were placed in a large room, in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet: and, being ſuffered to remain in this ſituation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as poſſible, and the contents of the bottles A and B appearing, by their incloſed thermometers, to be exactly at the ſame temperature, the bottles were all wiped with a very clean, dry, cambric handkerchief; and, being afterwards ſuffered to remain expoſed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat, or of the moiſture attached to their ſurfaces, which might have been occaſioned by the wiping, might be corrected by the operation of the atmoſphere by which they were ſurrounded, they were all weighed, and were brought into the moſt exact equilibrium with each other by means of ſmall pieces of very fine ſilver wire attached to the necks of thoſe of the bottles which were the lighteſt.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were ſuffered to remain, perfectly at reſt and undiſturbed, forty-eight hours; the bottles A and B being ſuſpended to the arms of the balance, and the bottle C ſuſpended, at an equal height, to the arm of a ſtand conſtructed for that purpoſe, and placed as near the balance as poſſible, and a very ſenſible thermometer ſuſpended by the ſide of it.

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At the end of forty-eight hours (during which time the apparatus was left in this ſituation) I entered the room, opening the door very gently for fear of diſturbing the balance; when I had the pleaſure to find the three thermometers, *viz.* that in the bottle A, which was now incloſed in a ſolid cake of ice, that in the bottle B, and that ſuſpended in the open air of the room, all ſtanding at the ſame point, 29° F., and the bottles A and B *remaining in the moſt perfect equilibrium.*

To aſſure myſelf that the play of the balance was free, I now approached it very gently, and cauſed it to vibrate; and I had the ſatisfaction to find, not only that it moved with the utmoſt freedom, but alſo, when its vibration ceaſed, that it reſted preciſely at the point from which it had ſet out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewiſe remained of the ſame apparent weight as at the beginning of the experiment, being in the ſame perfect equilibrium with the bottle A as at firſt.

I afterwards removed the whole apparatus into a warm room, and, cauſing the ice in the bottle A to thaw, and ſuffering the three bottles to remain till they and their contents had acquired the exact temperature of the ſurrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated ſeveral times, and always with preciſely the ſame reſult; the water, *in no inſtance*, appearing to gain or to loſe the leaſt weight upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the leaſt changed, by the various degrees of heat, and of cold, to which they were expoſed.

If the bottles were weighed at a time when their contents were not *preciſely of the ſame temperature*, they would frequently appear to have gained, or to have loſt, ſomething of their weights; but this doubtleſs aroſe from the vertical currents which they cauſed in the atmoſphere upon being heated or cooled in it; or to unequal quantities of moiſture attached to the ſurfaces of the bottles; or to both theſe cauſes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat *directly*, and as its conducting power *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glass. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight upon being changed from a state of *fluidity* to that of *ice*, and *vice versâ*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, from the results of the above-mentioned experiments, that if heat be in fact a *substance*, or matter, (a fluid *sui generis*, as has been supposed,) which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in nowise be affected by such motion.

It

It is, no doubt, upon the ſuppoſition that heat is a ſubſtance diſtinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken with a view to determine the weight which bodies have been ſuppoſed to gain, or to loſe, upon being heated or cooled, have been made; and upon this ſuppoſition (but without, however, adopting it entirely, as I do not conceive it to be ſufficiently proved,) all my reſearches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable; in which no foreign cauſe whatever could affect the reſults of them; and the quantity of heat which water is known to part with, upon being frozen, is ſo conſiderable, that if this loſs has no effect upon its apparent weight, it may be preſumed that we ſhall never be able to contrive an experiment by which we can render the weight of heat ſenſible.

Water, upon being frozen, has been found to loſe a quantity of heat amounting to 140 degrees of Fahrenheit's thermometer; or, which is the ſame thing, the heat which a given quantity of water, previouſly cooled to the temperature of freezing, actually loſes, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water at the given temperature, (that of freezing,) would heat it 140 degrees, or would raiſe it to the temperature of (32° + 140) 162° of Fahrenheit's thermometer, which is only 60° ſhort of that of boiling water; conſequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loſes almoſt as much heat as, added to it, would be ſufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid ſtate, and heated to the temperature of 61° F., and by the ice, in the experiments before-mentioned, was *at leaſt* nearly equal to that between water in a ſtate of boiling, and the ſame at the temperature of freezing.

But this quantity of heat will appear much more conſiderable, when we conſider the great capacity of water to contain heat, and the great apparent effect which the heat that water loſes upon being frozen would produce, were it to be imbibed

bibed by, or communicated to, any body whofe power of receiving and retaining heat is much leſs.

The capacity of water to receive and retain heat, or what has been called its ſpecific quantity of latent heat, has been found to be to that of gold as 1000 to 50, or as 20 to 1; conſequently, the heat which any given quantity of water loſes upon being frozen, were it to be communicated to an equal weight of gold at the temperature of freezing, the gold, inſtead of being heated 162 degrees, would be heated $140 \times 20 = 2800$ degrees, or would be raiſed to a *bright red heat*.

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about 9½ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no ſenſible effect upon a balance capable of indicating ſo ſmall a variation of weight as that of $\frac{1}{1000000}$ part of the body in queſtion; and if the weight of gold is neither augmented nor leſſened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very ſafely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES WILL BE FRUITLESS.

X. *Extract from the Report reſpecting Experiments made at the Polytechnic School in the Years V. and VI. on the Combuſtion of the Diamond.* By C. GUYTON.

[Concluded from Page 61.]

Repetition of the Experiment in Fructidor, Year VI.

AFTER examining what means were moſt proper for preventing the veſſels from cracking by the inequality of their dilatation, it was found that there were none more certain than to employ, inſtead of a glaſs bell, a well-choſen globe of a moderate thickneſs, and of ſuch a ſize that its ſurface might be at a ſufficient diſtance from the point of the luminous cone.

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The globe which we judged beſt for anſwering theſe conditions was 28·63 centimetres in diameter: its content was 123·25 decilitres, or 12,325 cubic centimetres, comprehending that portion of the neck which it was thought proper to retain, and which was 159 millimetres. That we might the more eaſily obſerve the riſing and falling of the mercury in the inſide, and thence determine the volumes of gas, we cemented on the outſide ſlips of paper, on which we traced, by means of meaſured veſſels, ſcales indicating decilitres, or or 100 cubic centimetres.

It may be readily conceived, that it was not poſſible to fill ſo frail a veſſel with mercury in order to diſplace it afterwards by oxygen gas. In transferring the gas by means of water, we ſhould have been obliged to leave a portion of that liquid on the interior ſide of the veſſel. We determined, therefore, to convey the gas, at the moment when it was diſengaged from the oxygenated muriat of pot-aſh, by means of a tube adapted to the diſtilling apparatus, and made to deſcend to the bottom of the globe in ſuch a manner that the common air ſhould be forced to iſſue from the globe by another pipe fixed in the ſtopper of the oriſice, and communicating with the pneumatic tub.

This proceſs is exactly the inverſe of that which I propoſed in my work on aëroſtats *, to fill a balloon of inflexible matter with hydrogen gas. It was founded on the ſame principle—the difference of the ſpecific gravity of the two fluids. Here it had the advantage of leaving the veſſel perfectly clean; an important condition, and which it is ſo difficult to obtain when air is expelled by mercury.

It was readily foreſeen that the firſt portions of the oxygen gas would become mixed with the atmoſpheric air, and that it would be neceſſary to diſplace this mixture ſeveral times by new quantities of oxygen gas, that no more azotic gas might remain in it, or, at leaſt, that the remaining quantity ſhould be ſo ſmall as to be incapable of having a ſenſible effect on the reſults of the experiment. We had even contrived means to determine it, by receiving under the pneumatic bell the

* Deſcription de l'aëroſtat de Dijon, &c

laſt portions diſplaced, that we might ſubject them to a eudiometric proof.

With this view we employed 18 decagrammes (about ſix ounces) of the oxygenated muriat of pot-aſh, which were put into a retort, at once to furniſh, at one operation, the whole quantity of the gas neceſſary for this renewal.

Thoſe who have not tried themſelves this method of ſubſtituting one aëriform fluid for another, might entertain ſome doubt reſpecting the purity of that employed in our experiment; but it will be eaſy for me to remove it. This was an article of ſo much importance that we could not neglect attempting to collect proofs of it.

We know with what ſucceſs M. Humboldt applied to the improvement of eudiometry. The intereſt which he took in our experiment induced me to invite him to come and determine himſelf with thoſe inſtruments, and by thoſe proceſſes, which were familiar to him, the purity of the oxygen gas in which the combuſtion was to take place. He readily accepted my invitation, as I had reaſon to expect, from his well known zeal for the progreſs of ſcience; and this article of our report is the production of his pen. I had likewiſe the ſatisfaction of ſeeing him apply eudiometric inſtruments to the examination of the reſiduum of the gas after the combuſtion. The following is the reſult of the proofs to which he ſubjected that introduced into the apparatus to ſerve for the combuſtion:—

Nitrous gas, diſengaged, by means of copper, from weak nitric acid, was tried with ſulphat of iron and oxygenated muriatic acid, which ſhewed in it from 0·09 to 0·10 of azot. One hundred parts of oxygen gas received, towards the middle of the operation, as it iſſued from the globe, in which we had propoſed to diſplace, in ſucceſſion, common air by oxygen gas, were mixed with 300 parts of this nitrous gas: there was an aëriform reſiduum of 0·66: making allowance for 0·27 or 0·30 of azot, pre-exiſting in the nitrous gas, we judged that, in 100 parts of gas tried, there were ſtill 36 of azotic gas.

We then continued to introduce into the globe freſh oxygen gas. We collected towards the end a portion which was

ſubjected to the ſame proof, in the ſame proportions, with the ſame nitrous gas. The reſiduum this time was only 30 parts; and as the 300 of nitrous gas employed contained nine or ten parts for $\frac{9}{10}$ of azot, we concluded that this oxygen gas was quite pure.—Theſe are the words of M. Humboldt.

When this firſt condition was fulfilled, the queſtion then was to place, in the centre of this globe, the diamond deſtined for the experiment. We had previouſly formed a ſmall cup of the lower portion of a furnace-pipe, the tube of which, five centimetres in length, was fixed to an iron ſtalk, and this ſtalk was ſtuck into a cylinder of cork deſtined to be inſerted in the neck of the balloon. This cork was dipped in maſtic to ſhut its pores, and a ſmall glaſs tube paſſed through it to eſtabliſh a communication between the inſide of the balloon and the mercurial tub. (See fig. 2. Plate II.)

We then placed the diamond on the cup, where we left it, having put it there by means of a ribbon, which we drew from under it in an inſtant, as ſoon as the balloon had been inverted, and its neck immerſed in the mercury. The balloon in this poſition was made faſt in a kind of collet, which reſted on the edges of an iron mortar that ſerved as a hydrargyro-pneumatic tub. We then exhauſted, by ſuction, a portion of the oxygen gas ſufficient to make the mercury riſe to 12 centimetres above the orifice of the balloon.

The diamond was the ſame that had been already expoſed to the action of the ſolar fire towards the end of the experiment of the preceding year, and which had loſt only two deci-milligrammes, and conſequently weighed 199.9 milligrammes (3·766 grains), or one carat wanting $\frac{1}{3\frac{1}{2}}$ *.

On the 5th Fructidor laſt year, at one in the afternoon, we began to throw upon the diamond the focus of the large lens of the National Inſtitute. The thermometer, expoſed to the ſun under a bell-glaſs, indicated 39·75: the mercury in the barometer ſtood at 75·89 centimetres, (28 inches 0·5 lines.) The volume of air, incloſed by means of the mercury in the tub, brought to the preſſure of 757·7 millime-

* It is well known that the carat of the jewellers is only 205·72 milligrammes.

tres (28 inches), and to the mean temperature of 12·5 degrees of the decimal thermometer, according to the experiments of Cit. Pneier, and the tables of Cit. Prony *, was then found to be 11,470 cubic centimetres.

Having taken the neceſſary precautions to heat gradually the balloon, the point of the luminous cone being almoſt in the centre, we were obliged to cover with a glaſs plate the wooden ſupporter, which was already on fire. The diamond firſt exhibited a black point at the angle immediately ſtruck by the ſun. We afterwards ſaw it entirely black, and as it were charred: we diſtinctly perceived, a moment after, brilliant points in a ſtate of ebullition, as it were, on the black ground. The ſolar rays, having been for a moment intercepted, it appeared tranſparently red. The ſun becoming obſcured by a cloud, we ſaw it of a much purer white than it had been at the commencement of the operation.

The ſun having emerged from the cloud, the ſurface of the diamond aſſumed the appearance of metallic ſplendour: it was then ſenſibly diminiſhed, and there remained no more than a quarter, of a lengthened form, without angles or perceptible edges, but ſtill very white, and of a beautiful tranſparency. We obſerved a ſlight fiſſure at the bottom of the pipe which ſupported it, but without any ſeparation of the parts. I muſt not forget, that at the commencement of the combuſtion we thought we obſerved a purpureſcent cone ariſing from the ſupport in the pencil of the ſolar rays; but this phenomenon was only an optical effect, which depended on the poſition of the obſerver.

The whole apparatus was left in the ſame ſtate, only defended by an inverted box placed over it, until the 7th, when we again began, at one hour twenty minutes, to preſent the diamond to the focus. We ſoon obſerved the ſame phenomena as on the 5th, the black ſurface, the brilliant points in ebullition, which vaniſhed and re-appeared according to the intenſity of the focus: we ſaw alſo a brilliant metallic appearance, or rather leaden-colour. This is the expreſſion which the aſſiſtants employed to characteriſe this phenomenon. At one hour forty minutes the diamond was entirely

* Journal Polytechnique, Part II. p. 65.

conſumed.

conſumed. We at firſt ſuſpected that there ſtill remained a brilliant particle; but we ſoon judged that it was a vitrified point of the ſupport, which was confirmed on inſpecting the pipe when drawn from the globe. The queſtion now was to collect the products of the combuſtion. No means ſeemed likely to be attended with more certainty than to introduce water of barytes into the apparatus, taking care to adhere, as nearly as poſſible, to the proportions indicated for the ſaturation of the carbonic acid, which we ſuppoſed muſt have been formed to prevent the uncertainty which the exceſs of this re-agent might occaſion in the reſults. The whole apparatus being removed into the ſhade, we began on the 9th to draw out the pipe which ſerved as a ſupport, and obſerved on it two ſlight fiſſures occaſioned by the contraction, and a ſpot of four or five millimetres in diameter, the centre of which had a vitreous appearance, and its edges a reddiſh hue. On examining it with a magnifying glaſs, we obſerved at the loweſt point a ſpace of two or three millimetres diameter, the ſurface of which was really vitrified, but of a tarniſhed and unequal colour. We diſtinguiſhed a particle of white glaſs, pure and brilliant, formed into a globule ſome ſmaller portions of the ſame nature, and two ſmall globules of a vitreous ſubſtance, which had a greeniſh red colour.

On one ſide we obſerved on the edges ſeveral other very ſmall globules interſperſed in a yellowiſh ground, and on the oppoſite a ſlight tinge of very bright red with very ſmall reddiſh points. A particle of white earthy matter was at firſt taken for a fragment detached from the edges of the pipe, but it was found friable, and aſcertained, by the ſtain it left on gold, to be oxyd of mercury. This examination being finiſhed, we introduced into the globe five meaſures of ſaturated water of barytes, each of 46·5 cubic centimetres. The liquor immediately aſſumed a milky appearance, and there was a diminution in the volume of gas, which, calculated by means of the attached paper ſcale, amounted nearly to 300 cubic centimetres. Thus we might ſtop here and conſider the experiment as terminated, and by making ſome allowance for the errors unavoidable in ſuch manipulations, and make the reſult tally with the amount, determined before by the noble experiment of Lavoiſier and La Place, of the re-

ſpective quantities of carbon and oxygen which form the carbonic acid. Mr. Tennant ſeems to have done the ſame thing lately after the combuſtion of the diamond by nitre. But we ſhould only have confirmed what was before known, or ſuppoſed to be known. Our object was not only to obſerve, with more attention, what took place during the act of combuſtion, but to aſcertain, as accurately as poſſible, the nature and quantity of the product, and the reader will find that the labour undertaken on this ſubject has not been fruitleſs.

The liquor was agitated in the globe to mix the white matter which had been depoſited. We drew out $4\frac{1}{2}$ meaſures of the five we had introduced by making uſe of the ſame inverted bottle filled with mercury, and which we raiſed on the inſide by means of an iron ſtalk compoſed of ſeveral pieces, which could be adjuſted by ſcrews. (See fig. 6. Plate II.)

We introduced into the balloon three new meaſures, each containing the ſame quantity of diſtilled water, which was ſhaken in the inſide to detach and collect what adhered to the ſides. Theſe united liquors, being immediately filtered in an open filtre, left 192 centigrammes (36·142 grains) of carbonat of barytes dried in the heat of boiling water.

It may be readily judged what was our aſtoniſhment when proceeding to examine the liquor, inſtead of finding in it a ſlight exceſs of uncombined barytes, we obſerved that it changed neither the colour of turmeric nor that of logwood, and that, on the contrary, it acted on an infuſion of turnſole as water charged with the carbonic acid. The preſence of this acid unequivocally manifeſted itſelf, when we poured upon it a few drops more of barytes water, which immediately rendered it turbid. It was neceſſary to add even 4·65 centimetres of this water to ſaturate and precipitate the remaining acid gas. Being informed by this phenomenon that the production of the gas had been more conſiderable than we expected, and that ſome of it ſtill remained mixed in the aëriform fluid in the balloon, we took every meaſure neceſſary to determine the quantity. This we were luckily enabled to do by the diviſions which had been marked on the ſcales, the orifice of the globe having never been yet taken out of the mercury.

When the barytes water was taken out, the apparent volume

lume was found to be exactly 122 decilitres, the internal column of the mercury above the level of the tub was 47 millimetres; the barometer being at 759·96 millimetres, (Fructidor 19, year VI.) the centegrade thermometer at 21·25, the real volume, at a mean preſſure and temperature, was 112.426 decilitres, or 11242·66 cubic centimetres.

I ſtill invited M. Humboldt to co-operate with us in examining the nature of this reſiduum of gas. It was transferred in his preſence into a pneumatic tub prepared on purpoſe with diſtilled water, and received into four large flaſks. The trial was made by the ſame inſtruments, and with the ſame nitrous gas, which had ſerved for the oxygen gas before the combuſtion, and conſequently containing from 0·09 to 0·10 of azotic gas. The trials made on portions extracted from different flaſks varied from 31 to 34 in the quantity of the reſiduum of gas, in a mixture of 100 parts of gas examined with 300 parts of nitrous gas.

I ſhall not even take the mean term, I ſhall ſtop at the weakeſt, which indicates four hundredth parts of carbonic acid gas, which, I think, I can aſſert to be rather below than above the truth; ſince a portion of this ſame gas, brought into contact with ammonia, under a receiver, experienced a diminution of 4·5 *per cent.*

Let us now eſtimate the carbonic acid gas which entered into the compoſition of the 192 centigrammes of carbonat of barytes. According to Pelletier, whoſe accuracy is well known in reſearches of this kind, 100 of this earthy ſalt contain 22 of acid gas *, which gives 42·24 for 192; and as the cubic centimetre of gas weighs 1·847 milligrammes, it follows that the 42·24 centigrammes repreſent 228·621 cubic centimetres. If we now add, on the one hand, the 449 cubic centimetres, found in the reſiduum of the gas after combuſtion, and which, as we ſaw, formed the four hundredth parts; and deduct, on the other, the ſame quantity from the aëriform fluid in which the combuſtion was effected, it reſults, that in 11470 cubic centimetres of oxygen gas contained in the balloon, there remained, after the combuſtion, only 10793; that 677 were conſumed; that theſe 677 cubic centimetres of

* *Annales de Chimie*, Vol. XXI. p. 135.

oxygen

oxygen gas, in the ratio of 1·3577 milligrammes each, produced, with the 199·9 milligrammes of the diamond, 1117·96 milligrammes of carbonic acid.

In the laſt place, that, inſtead of the proportions 0·28 of combuſtible ſubſtance, and 0·72 of acidifying principle, obſerved in the combuſtion of carbon, the proportion was, for the combuſtion of the diamond, - 17·88 of carbon.
82·12 of oxygen.

100·00

Though it was not poſſible for me to doubt facts deduced from calculation, I at firſt heſitated to admit differences ſo conſiderable in the manner in which the ſame combuſtible united itſelf to oxygen in the quantities it could take up, and the products of its combuſtion; in a word, a carbonaceous combuſtible more abundant in real combuſtible matter than charcoal itſelf, and which at the ſame time differed ſo much from it in the degree of temperature neceſſary to determine the action of its affinity. But I ſoon began to reflect, 1ſt, That this would not be the only inſtance of the firſt degree of the oxydation of an acidifiable baſe having been operated with great difficulty, while the acidification was afterwards completed with the utmoſt facility: 2d, That ſeveral ſubſtances of the ſame kind preſented to us alſo theſe two characters; a greater abundance in real carbon, and greater reſiſtance to inflammation; ſo that they naturally placed themſelves in an intermediary rank between the diamond and charcoal. Theſe two conſiderations, ſtill ſtrengthened by the ſimilarity of the phenomena obſerved during the courſe of our two experiments in the paſſage of the diamond to the ſtate of carbonic acid, appeared to me to throw a ray of light on this ſubject hitherto ſo obſcure.

In regard to the firſt conſideration it will be ſufficient for me to call to mind with what difficulty the commencement of a compoſition of azot and oxygen is formed by the direct way, and the high degree of temperature which it requires, while nitrous gas cannot be in contact with oxygen without paſſing immediately to the acid ſtate. Charcoal will then be to the carbonic acid what nitrous gas is to the nitric, and the

the diamond will be to charcoal what azot is to nitrous gas. There will, therefore, be no longer occaſion of wonder that more oxygen is neceſſary to that ſubſtance, which as yet has none of it, than to that which has already been united with the quantity neceſſary for arriving at the firſt point of ſaturation.

The ſecond conſideration reſts on facts no leſs concluſive. Plumbago is a carbonaceous combuſtible, which does not burn but at a very high temperature, or in nitre in fuſion; which produces by its combuſtion carbonic acid; which, as well as the diamond, is more abundant in combuſtible matter than carbon itſelf. We are indebted to the illuſtrious Scheele for the firſt obſervation of this fact. One part of carbon alcalifes only five parts of nitre; one part of plumbago can alcalife ten. The operation performed in a retort on 80 centigrammes of plumbago, gave him 357 cubic centimetres of carbonic acid gas*. This agreement will not be conteſted by thoſe who, having been witneſſes of our experiment, ſo unanimouſly declared, that the ſurface of the diamond aſſumed inſtantaneouſly a leaden colour.

This mineral is not the only body which preſents theſe ſtriking characters of a ſubſtance almoſt incombuſtible, and yet very abundant, in combuſtible matter. I deſcribed, ſixteen years ago, in the Memoirs of the Academy of Dijon, a foſſil found in a maſs in the coal-pits of the Rive-de-Gier, which was ſent to me under the name of *incombuſtible coal*, and which I then conſidered as real coal which had paſſed to the ſtate of plumbago. I characteriſed it in that manner.

Our brother Dolomieu has deſcribed a foſſil of the ſame kind, which he calls *carbure of alumine*, which is the *anthracolite* of Werner. I had already ſuſpected that it was neither the preſence of four or five centiemes of alumine, nor that of a ſtill ſmaller quantity of iron, that rendered it incombuſtible, but the little advanced ſtate of the oxydation of the carbon. I ſubjected it to two experiments, by which this was fully confirmed.

The object of the firſt was to determine if the alumine preſent was in a ſtate of combination ſufficiently intimate to

* Mem. de Scheele, French edit. Vol. II. p. 27 and 29.

reſiſt the action of pot-aſh by the humid way: 100 parts, put in digeſtion in that ſolvent, left in it 4·6 of alumine.

The ſecond was, to aſcertain whether this combuſtible, which poſſeſſed ſo little inflammability, had alſo the power to alcaliſe more nitre than carbon, conſequently to take up more oxygen. Three ſucceſſive trials gave for a mean reſult the alcaliſation of 7·87 parts of nitre by one part of that mineral; and the ſame coally matter, digeſted for four or five days in oxygenated muriatic acid, burnt completely with 6·5 of nitre.

M. Klaproth, the celebrated chemiſt of Berlin, had before ſubmitted to trials of the ſame kind a foſſil deſcribed by M. Widenmann under the name of *incombuſtible coal*, and found that 100 parts left, after combuſtion at a very ſtrong heat, only ſeven of a cineritious reſiduum; that treated in a crucible with eight parts of nitre, and the maſs diſſolved in water, acids occaſioned no precipitate. Kirwan, in his experiments on coal, remarks, that that which he calls *Kilkenny coal*, and which has a metallic brilliancy, which does not burn but when carried to incandeſcence, and which then conſumes ſlowly without emitting flame, can decompoſe 9·6 of nitre.

After this I do not ſee how there can remain any doubt that theſe ſuppoſed incombuſtible ſubſtances are real oxydes of carbon, which, like coal or charcoal, have the property of conducting the electric fluid; of cementing iron; of taking the oxygen from ſome acidifiable baſes; but which are not at that degree of oxydation neceſſary for exerciſing this ſeparating affinity at a weak temperature.

I muſt not omit this opportunity of making ſome application of this principle, which may become uſeful to the arts. It has not yet been ſufficiently explained, why ſome animal and vegetable matters produce carbon ſo difficult to be incinerated; why charred pit-coal, known under the name of coke, or cinders, and which has been half burnt in the preparation, is, however, ſo powerful a combuſtible; why peat, or turf, the weakeſt of combuſtibles, acquires, by being properly charred, the property of welding large pieces of iron better than charcoal; and why, in the laſt place, charcoal, when expoſed to a very ſtrong heat in veſſels impenetrable

to

to air, becomes there, in a certain degree, incombuſtible, as is proved in the experiment made by Mr. Tennant, which I have mentioned in the article Air in the *Dictionnaire de Chemie Encyclopedique*, Vol. I. p. 714.

The anſwer to all theſe queſtions may be found in the theory I have laid down: they are charcoals in the firſt degree of oxydation. Thus ſome of them have not yet acquired that which conſtitutes charcoal properly ſo called; others, after poſſeſſing all the qualities of vegetable and mineral carbon, have returned to the firſt degree by a real (*debrulement*) unburning of the remaining carbon; ſo that by loſing their inflammability they become capable of fixing a greater quantity of oxygen, and conſequently of ſetting at liberty a greater quantity of caloric when they find themſelves at a temperature ſufficiently high to determine and complete their acidification.

Some practical conſequences will doubtleſs hence be deduced in regard to proceſſes for the reduction of metals; for the cementation of ſteel, which it is probable takes up only oxyd of carbon, ſince it is ſeparated from it in that ſtate; for the incineration of the carbonaceous reſiduums of our analyſes; for the carboniſation of wood, pit-coal, and turf: in a word, we may perhaps thence conclude the poſſibility of rendering uſeful thoſe maſſes of pit-coal, ſaid to be incombuſtible, found at Rive-de-Gier, by mixing it with more inflammable matters, to maintain the temperature which determines its combuſtion. Its poſition, texture, and all its exterior characters announce, as already ſaid, that it conſiſts of beds of coal changed by a ſubterranean fire; and this is confirmed by tradition, which preſerved to that mountain, for three centuries, the name of the *Mountain of Fire.* (*Montagne de Feu.*) We can now pronounce that it is coke too far advanced, but ſo much the more ſuſceptible of producing a great heat under favourable circumſtances.

RECAPITULATION.

I ſhall here enumerate the conſequences, or rather the facts, which reſult from the phenomena obſerved in the two

combuſtions of the diamond by the ſolar fire, and the experiments which followed:—

1. It is not only by the colour, weight, hardneſs, tranſparency, and other ſenſible characters, that the diamond *differs* from charcoal, as ſeems hitherto to have been believed;

2. Nor is it by the ſtate alone of the aggregation of the matter that conſtitutes diamond:

3. Neither is it on account of the 200th part of the cineritious reſiduum left by carbon, or the ſmall quantity of hydrogen which it contains.

4. It is more eſſentially by its chemical properties that it differs.

5. The diamond is the pure combuſtible ſubſtance of this genus.

6. The product of its combuſtion, or of its combination with oxygen to ſaturation, is carbonic acid without reſidue.

7. Carbon burns at a temperature eſtimated at 188° of the centigrade thermometer; the diamond does not inflame but at about 30 pyrometric degrees, which, according to Wedgewood's ſcale, makes a difference of 188 to 2765.

8. Charcoal, when kindled, maintains of itſelf, in oxygen gas, the temperature neceſſary for its combuſtion. The combuſtion of the diamond ſtops when you ceaſe to maintain it by a furnace-heat, or the union of the ſolar rays.

9. The diamond, for its complete combuſtion, requires a much greater quantity of oxygen than charcoal does, and produces alſo more carbonic acid. One part of charcoal abſorbs in this operation 2·527 of oxygen, and produces 3·575 of carbonic acid. One of diamond abſorbs a little more than four of oxygen, and really produces five of carbonic acid.

10. There are ſubſtances which are in a ſtate of intermediary compoſition between the diamond and charcoal. Theſe are plumbago, or native carburet of iron; incombuſtible foſſil coal; the carburet of alumine of Dolomieu; the anthracolite of Werner; the black matter united to iron in the ſtate of caſt iron and ſteel; carbonaceous reſiduums difficult to be incinerated; and carbon itſelf unburnt, (*débrulé*,) by the action of a ſtrong heat without the contact of air.

11. Theſe ſubſtances mixed, or weakly combined with three or four hundredths of their weight of iron, or alumine, give by their combuſtion carbonic acid, like charcoal and the diamond.

They approach to carbon by their colour, their lightneſs, their opacity; by their ſerving, like it, to decompoſe water, to cement iron, to deoxydate metals, to deoxygenate ſulphur, phoſphorus, and arſenic; and by conducting, like it, the electric fluid. They approach the diamond by containing more combuſtible matter than charcoal; by abſorbing alſo more oxygen, and producing more carbonic acid; by decompoſing more nitrous acid; by burning only at a much higher temperature, even in nitre in fuſion; and by their combuſtion being ſtopped when this temperature is lowered. They ſeem to differ from each other by the property of producing with zinc galvanic irritation, as well as ſilver does: which can be effected neither by the diamond nor charcoal.

12. Thus the diamond is pure carbon, the pure acidifiable baſe of the carbonic acid. Its combuſtion is effected in three periods, which require three different temperatures. At the firſt, which is the higheſt, the diamond aſſumes a black leaden colour. It is an oxydation in the firſt degree, the ſtate of plumbago and anthracolite. At the ſecond temperature, which may be eſtimated at 18 or 20 pyrometric degrees, there is a ſecond flow and ſucceſſive combination of oxygen. It is a progreſs of oxydation which conſtitutes the habitual ſtate of charcoal, or rather that in which it is found after the action of a ſtrong heat in cloſe veſſels has diſengaged a part of its oxygen.

Thus plumbago is an oxyd of the firſt degree, charcoal an oxyd in the ſecond, and the carbonic acid the product of the complete oxygenation of the carbon.

Suppoſing, then, that we operate with ſufficient preciſion to take away from the ſurface of the diamond the black matter in proportion as it is formed, by ſuddenly withdrawing from it each time the action of the ſolar fire, we ſhould doubtleſs be able to convert it into charcoal, or at leaſt plumbago, if the too rapid paſſage of the laſt degree of oxydation to oxygenation did not prevent us from ſurpriſing it in that ſtate.

13. In the laſt place, ſeveral conſequences, of importance to chemiſtry and the arts, ariſe from theſe principles.

After this concluſion it will be aſked, no doubt, how it happens that the ſimple matter, pure carbon, the diamond, is ſo rare while its compounds in different ſtates are ſo abundantly diffuſed? To put an end to the aſtoniſhment of thoſe who might entertain any miſtruſt, I ſhall obſerve, that aluminous earth is alſo one of the moſt common matters, and that adamantine ſpar, as rare as the diamond, is however only alumine; that iron every where exiſts, under all forms, except in the ſtate of purity: the exiſtence of native iron is ſtill doubtful. The wonder exiſts only in the oppoſition of facts to our opinions, and will diſappear in proportion as we diſcover, and appropriate to ourſelves, the means employed by nature in producing the ſame effects.

Thoſe who have never turned their attention to the phyſical ſciences, to eſtimate at leaſt their influence on public felicity, are diſpoſed to treat as vain curioſity labours which are not immediately directed towards a near object of new enjoyment. What would have been their aſtoniſhment had they been told, that reſearches on the nature of the diamond would one day produce truths which might give riſe to happy changes in the practice of the moſt familiar arts; in the preparation, and in the employment of the coarſeſt combuſtibles! Such, however, are the conſequences that may ariſe from the beſt known properties of the eſſential principle carbon in its different ſtates.

XI. *A Letter to the Editor, containing ſome Objections to the Mitchillian Theory of Peſtilential Fluids.*

SIR,

I OBSERVE, in the 15th Number of your uſeful Magazine, a communication from Dr. Mitchill, of New-York, ſhewing the utility of conſtructing the houſes, and paving the ſtreets of cities, with *calcareous* in preference to *ſiliceous* and *argillaceous* materials. This communication is contained in a letter from Mr. Da Coſta to the Doctor, in which it

it is obſerved, that although the city of Liſbon is one of the filthieſt in Europe, and the moſt infeſted with putrid effluvia, yet that it nevertheleſs is remarkable for its ſalubrity. This is attributed by Mr. Da Coſta to the calcareous materials, of which the buildings and pavements are conſtructed, abſorbing and neutraliſing the ſeptic fluids, the cauſe, according to Dr. Mitchill's theory, of all contagion.

In reply to this I would obſerve, that from the author's own account it is evident that the calcareous earth in the city of Liſbon does *not* deſtroy the putrid exhalations in any ſenſible degree, and therefore cannot prevent any diſorders to which they may give riſe. The houſes, he obſerves, are very offenſive, from the privies, and from inattention to cleanlineſs. The mouths of the ſewers go to the wharves, and are bare at low water. The fluid which comes from theſe contains ſo many infectious matters that its *ſtrong putrid ſmell can ſcarcely be endured!* It is plain, therefore, that the putrid exhalations are not deſtroyed, or, at leaſt, that they are ſo quickly generated as to annoy the olfactory ſenſe in an intolerable degree. The bodies of the inhabitants are conſtantly immerſed in putrid exhalations, yet contagion is not produced. The inference is, that putrid exhalations are not its immediate cauſe. Contagion ariſes where no putrefaction is going on, at leaſt in any degree perceptible to the ſenſes; as when a number of perſons are crowded together in jails, and even in the houſes of the poor in the winter ſeaſon, without ſufficient ventilation. On the other hand, putrefaction may be preſent, in a very high degree, without giving birth to contagion, as may be obſerved in ſlaughter-houſes, cat-gut manufactories, &c.

But, is it proved ſufficiently that calcareous matters do abſorb and neutraliſe ſeptic exhalations? Mr. Da Coſta remarks, that he has obſerved, two or three times, in Liſbon, dead animals upon the ruins of houſes, and of courſe ſurrounded by calcareous earth, in a ſtate of deſiccation; and, at the ſame time, two or three fathoms diſtant, another animal dead too, and lying upon another kind of ſoil, in a ſtate of complete putrefaction. I do not preſume to queſtion the truth of this, ſince it was a matter obvious to the ſenſes: but I deny

I deny the generality of the fact equally from obſervation. I ſuſpended a piece of fleſh, in a proper veſſel, over a layer of chalk, and at the ſame time laid chalk, in ſmall pieces, on gauze, a few inches above the fleſh: but I did not obſerve that putrefaction was more backward than in other circumſtances, or that putrid exhalations were leſs extenſively diffuſed around. That quickline deſtroys putrefaction, is no proof of the opinion here advanced, ſince this can be accounted for on other principles.

If fevers abound on the oppoſite ſide of the Tagus, whilſt the city of Liſbon is free from them, there are probably other cauſes to which this may be aſcribed, ſuch as a marſhy ſoil, &c. Whether any thing of this ſort exiſts, the account here given by Mr. Da Coſta does not enable us to judge.

It is the tendency of Dr. Mitchill's theory to overturn ſettled opinions, and to cauſe us to abandon practices, which have been generally employed, and much relied on, for the deſtruction of contagious matter. It is of great importance, therefore, that it ſhould be well founded, leſt we be induced to lay aſide means that are efficacious on grounds not ſufficiently eſtabliſhed. Whatever becomes of Dr. Mitchill's hypotheſis, it appears to me to receive no ſupport from the facts and arguments of the paper in queſtion, although it is conſidered by him as affording a ſtrong confirmation of its truth.

Wiſhing every ſucceſs to your valuable publication, I remain, Sir, yours, &c.

H. CLUTTERBUCK.

Walbrook, Oct. 21, 1799.

XII. *Deſcription of Mr.* HOWARD's *Improved Air-Furnace.*

THE difficulty of obtaining a degree of heat ſufficiently intenſe for many operations in chemiſtry, has been felt and lamented by every one engaged in ſuch purſuits; and, notwithſtanding the reſearches and numberleſs experiments made profeſſedly for that purpoſe by the greateſt men, the beſt conſtruction of an air-furnace is ſtill a problem; indeed,

deed, the rules laid down by thoſe who have written on this ſubject, differ ſo widely from each other, that we muſt ſuppoſe a great number of circumſtances, hitherto overlooked, enter into the conſtruction of a good furnace. Having obſerved effects produced in that of Mr. Edward Howard, of Doughty-ſtreet, which we have ſeldom, if ever, ſeen equalled in any other furnace, we thought we could not better oblige our chemical readers than by giving a deſcription of it. The difference in the preſent compoſition of Wedgewood's pyrometer pieces, from that uſed by him when he firſt invented his inſtrument, makes it impracticable (unleſs the ſcale was alſo altered) to meaſure correctly the degree of heat obtained; it muſt however be at leaſt = 160°, and is ſufficient to run down the beſt Heſſian crucibles. The moſt ſtriking deviation from the common conſtruction is in the lower part of the chimney, or flue, being ſmaller than the upper, and the greater depth of the aſh-pit. Mr. Howard thinks that ſomething depends on the direction of the horizontal funnel, which in this caſe opens to the north. If any of our readers ſhould conſtruct a furnace on this plan, we ſhould be glad to be informed of the coincidence of effect*.

Fig. 1. (Plate IV.) A, the cavity or body of the furnace, 9 inches ſquare and 1 foot 7 inches deep to the bars. B, the aſh-pit, 1 foot 2 inches, by 9 inches broad, and 2 feet 8 inches deep below the bars: this aſh-pit opens to the external air by an horizontal funnel C, 6 feet long, paſſing under the floor of the laboratory: this not only furniſhes a ſupply of denſer air, but prevents the unpleaſant effects of the cold draught on the legs and feet of the operator, which happens when the aſh-pit opens into the laboratory. The external opening of this funnel is about 2 feet ſquare, which gradually contracts to the aſh-pit in the manner ſhewn in the plate. D, the aperture of the horizontal flue, 7 inches wide by 2 deep, contracting in width to $4\frac{1}{2}$ inches, where it enters the vertical flue or chimney, which is $4\frac{1}{2}$ inches ſquare, of which width it continues to the height of 7 feet, and is then enlarged to 5 inches ſquare; which dimenſions it preſerves to

* Dr. Pearſon's furnace, which is a very powerful one, agrees in ſome parts of its conſtruction with Mr. Howard's.

the

the top of the brick-work, being 16½ feet from E. The chimney finiſhes by an iron pipe about 3 feet long.

Fig. 2, F, ſhews the plan of the body of the furnace 9 inches thick, the top courſe of bricks being bound together in the uſual manner by a ſtrong iron hoop G. H is an horizontal ſection of the chimney and flues*. When only a moderate degree of heat is wanted, the horizontal draught-hole C can be cloſed by a ſliding door or regiſter, and an opening made into the aſh-pit, directly under the bars, by removing the ſtone ſtopper I. (Fig. 1 and 2.) This alſo is uſeful for occaſionally clearing the bars from ſcoria, or entirely removing them, in order ſuddenly to put out the fire. The whole of the furnace and chimney is built of Windſor bricks.

XIII. *An Account of Mr.* BROWN*'s Travels through Egypt and Syria*, &c.

[Concluded from Page 76.]

MR. BROWN retired a ſecond time to Cobbé, with little hopes of ever leaving the country. Of the property, which the king's agents had purchaſed on his arrival, no part of the price had yet been paid. He had been inſulted with the mockery of juſtice, yet obliged to thank his oppreſſors for the compenſation which their corruption and malignity alone had rendered incomplete.

He had not omitted to renew to the Melek Muſa the requeſt which had been previouſly made to Miſellim and Ibrahim. He explained to him, in the leaſt exceptionable manner, his intention of coming to Cobbé; completely did away all the ſuſpicions which his enemies had at firſt excited; and concluded with deſiring permiſſion to go to Sennaar, or to accompany the firſt *ſelatea* (an armed expedition for the purpoſe of acquiring ſlaves,) to the ſouth or ſouth-weſt; or to have a ſafe conduct, and one of the Sultan's ſlaves, to accompany him to Bergoo, (the firſt Mahometan kingdom on the weſt.) By the firſt route he hoped to have reached Abyſſinia, or, if that had been impracticable, to have gone through Nubia to Egypt, or by Suakim to the Red Sea, and thence

* The chimney is detached about an inch from the wall behind all the way to the top.

to

to Mocha or Jidda. By the ſecond he was almoſt certain of ſettling ſome important points relative to the White River, poſſibly of tracing it to its ſource. And by the third, either of paſſing directly weſt, and tracing the courſe of the Niger, or of penetrating through Bornou and Fezzan to Tripoli.

To the firſt propoſal he anſwered, in a manner which gave Mr. Brown reaſon to doubt his ſincerity, that the road to Sennaar was unpaſſable, the Sultan being then maſter of only one half of Kordofân: that the natives of all that part of it which remained unſubdued were his implacable foes, and would infallibly deſtroy any perſon who came from Dar-Fûr: that he thought, however, if Mr. Brown waited another year, that route might poſſibly be more ſecure; and in caſe it ſhould, he would uſe all his efforts to obtain the Sultan's permiſſion for his departure.—Of the *Selatca* he ſaid, that our traveller would only encounter certain death by attempting it, as, between the jealouſy of thoſe who accompanied him, and the actual hoſtility of the country, there would be no hope of eſcaping. Mr. Brown hinted, that the Sultan might give him a few attendants, whom he was very ready to pay, and an order to enable him to paſs unmoleſted as his phyſician in ſearch of herbs. He replied, that he would propoſe ſuch a meaſure, but he did not expect it would receive the Sultan's approbation.—To the third propoſal he anſwered, that he had no hope of Mr. Brown's ſucceeding, and concluded with ſtrongly recommending to him to ſeize the firſt opportunity of returning to Egypt; but he aſſured him, if he could accompliſh any of the meaſures he ſo much wiſhed, he would not fail to inform him and to afford him the neceſſary aid. Such was the ſtate of affairs when Mr. Brown returned to Cobbé, dejected, and with little expectation of realiſing even his leaſt ſanguine hopes. Not more than ſix weeks after this converſation had taken place, he was ſent for to attend the Melek, who was confined by an old diſorder in his lungs. He found him yet ſenſible; but his eyes were fixed, and extremities incapable of motion. In five hours after, he expired. Thus were blaſted our traveller's returning hopes of ſucceſs; for no mediator now remained between him and the monarch, and no longer was there

near the court a man even of ſeeming liberality and good ſenſe, to whom his projects might be ſafely opened.

During the ſummer of 1794, five men who had exerciſed conſiderable authority in ſome of the provinces, were brought to El Faſher as priſoners; it was ſaid that they had been detected in a treaſonable correſpondence with the hoſtile leader, Haſhem, in Kordofân. They did not undergo any form of trial; but as the Sultan choſe to give credit to the depoſitions made againſt them, his command was iſſued for their execution. Three of them were very young men, the youngeſt not appearing to be more than ſeventeen years of age. A little after noon they were brought, chained and fettered, into the market-place, before one of the entrances of the palace, eſcorted by a few of the royal ſlaves armed with ſpears. Several of the Meleks, by the monarch's expreſs order, were preſent, to witneſs, as he termed it, what they might expect to ſuffer if they failed in their fidelity. The executioner allowed them time only to utter a ſhort prayer, when he plunged his knife into the neck of the oldeſt, exactly in the ſame manner as they kill a ſheep. The operation, too, is marked by the ſame term, *dhebbah*. He fell, and ſtruggled for ſome time. The reſt ſuffered in their turn. The three laſt were much agitated, and the youngeſt wept. The two firſt had borne their fate with becoming firmneſs. The crowd that had aſſembled had ſcarcely ſatiated itſelf with the ſpectacle of their convulſive motions while proſtrate in the duſt, when the ſlaves of the executioner coolly brought a ſmall block of wood and began mangling their feet with an axe. Having cut off their feet, they carried away the fetters which had been worn by the criminals, though of little value, and left the bodies where they were. Private humanity, and not public order, afforded them ſepulture.

Near the end of the year 1795 a body of troops was muſtered and reviewed, intended to replace thoſe who had died of the ſmall-pox in Kordofân, which, it was ſaid, amounted to more than half the army. The ſpoils which had been taken from Haſhem were alſo oſtentatiouſly diſplayed on this occaſion. They conſiſted of eighty ſlaves, male and female, but the greater proportion of the latter, many of whom were exceedingly

exceedingly beautiful, nor the leſs intereſting, that, though the change in their ſituation could not be very important, their countenances were marked with deſpondency. To theſe ſucceeded five hundred oxen and two hundred large camels; the whole proceſſion was cloſed by eighty horſes, and many articles of leſs value borne by ſlaves. Shouts rent the air of, Long live El Sultan Abd-el-rachmân el raſhîd! May God render him always victorious!

A ſhort time after, Mr. Brown cauſed a petition to be drawn up, which was preſented by Ali-el-Chatîb to the Sultan, in which he ſtated his ſufferings, requeſted payment of what yet remained due to him, and permiſſion to proceed on his journey to Kordofân. Though the perſon who preſented it was a man of conſiderable weight, no anſwer was given. He therefore followed it up by a viſit in perſon, which he had reſolved ſhould be his laſt. His arrival was no ſooner known than he was directed to attend ſome ſick perſon, as he had ſeveral times done before. This he poſitively refuſed, and it was many days before he could be admitted at court. On the 11th of December 1795, however, he accompanied the Chatîb to the monarch's preſence, and briefly ſtated what he came to requeſt; which the former ſeconded, though not with the zeal Mr. Brown wiſhed. To his demand of permiſſion to travel, no anſwer was returned. But the generous and hoſpitable monarch, who had received from him the value of 750 piaſtres in goods, though his claim was well ſupported, gave him only twenty meagre oxen, in value about 120 piaſtres! The ſtate of his purſe would not permit him to refuſe even this mean ſupply; and he bade adieu to El Faſher, as he hoped, for ever.

Having applied the value of the oxen to preparations for his journey to Egypt, the report of the caravan's departure growing daily ſtronger, he loſt no time in joining the Chabîr, who was then encamped at Le Haimer, (3d March 1796,) a ſmall village about three days journey north of Cobbé, where there was a tolerable ſupply of water, but no other requiſite for living.

Mr. Brown arrived at Le Haimer about a month before Ramadan; and it was not till the ſixth day of El Hedge, the

fecond month after that faſt, that they actually commenced their journey to Egypt. In the mean time, having pitched a tent under a great tree, where they were ſheltered from the rays of the ſun, he fed on polenta (*as-cidé*) and water with the camel-drivers. He had collected eight camels for the journey, but the beſt of them was ſtolen while grazing; another died; and to ſupply his place he was obliged to ſeek one on credit, for his whole exchangeable property at that time amounted only to eight piaſtres.

The journey, once commenced, was continued with little remarkable except violent heat. They returned by the only caravan route, *Bir el Malah*, *Leghea*, *Selime*, *Sheb*, and *El-wah*. Their proviſions were indifferent, and in ſmall quantity. The camel-drivers regaled themſelves with the fleſh of theſe animals when they chanced to be diſabled on the road. When they arrived at *Beiris*, they were met by a Caſhef, who welcomed the Jelabs with an exhibition of fire-works. On this occaſion it is cuſtomary to treat the chief merchants with coffee, and to preſent to each a *beniſh* of coarſe cloth, worth about a guinea; but he expects in return a ſlave from each, worth about ten guineas. When Mr. Brown arrived at Aſſiût, he had been four months without eating animal food; and the hard living, heat, and fatigue, had brought on a diarrhœa, by which he was much weakened; but before he left Aſſiût, where he paſſed twenty days, it was much abated.

After ſome ſtay in Egypt Mr. Brown embarked for Syria on the 19th of January 1797; and having viſited Jeruſalem, Damaſcus, Aleppo, and various other places, proceeded to Conſtantinople; from which he returned to England by Vienna, Dreſden, Leipſic, and Hamburgh. He arrived in London on the 16th of September 1798, after an abſence of nearly ſeven years.

XIV. *Tenth Communication from Dr.* THORNTON, *Phyſician to the General Diſpenſary*, &c. &c. &c. *relative to Pneumatic Medicine.*

ONE of the moſt dreadful of human afflictions is that where water oppreſſes the brain; and there are no hopes of any

any medicine getting to that part, except what firſt can enter the blood. Mercury has been ſucceſsfully employed; but it produces ſo great a derangement of the general health, that a ſubſtitute is certainly adviſable. Can the abſorbents of the brain be equally excited by vital air?

Lydia Johnſon, æt. 13, living at No. 5, Huſband-ſtreet, after a putrid fever became perfectly blind, and had frequent fits, evidently ariſing from oppreſſion of the brain. Various means had been employed without the leaſt advantage. I referred her to Meſſrs. Wathen and Phipps, that they might ſee the caſe. Under the inhalation of vital air, with the aid of bark and ſteel, the fits ſoon gave way; and afterward, finding the air produced no miſchief, I increaſed the quantity one day to twelve quarts, mixed with three times that quantity of atmoſpheric, and almoſt immediately after the inhalation ſhe had a dawning of ſight, and going home ſhe did nothing but cry out, Mama, mama! I ſee now every thing. The cure has remained permanent above a year, and I ſaw her the other day in perfect health and ſpirits.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ROYAL SOCIETY OF LONDON.

IN the courſe of the preſent month (November) the Second Part of the Tranſactions for the year 1799 was delivered to the Members. The contents are:—An account of the diſſection of an hermaphrodite dog: to which are prefixed, ſome obſervations on hermaphrodites in general. By Everard Home, Eſq. F. R. S.—An enquiry concerning the weight aſcribed to heat. By Benjamin Count Rumford, F. R. S. M. R. I. A. &c. —An account of ſome experiments on the fecundation of vegetables: in a letter from Thomas Andrew Knight, Eſq. to the

the Right Hon. Sir Joſeph Banks, K. B. P. R. S.—Obſervations on the different ſpecies of Aſiatic elephants, and their mode of dentition. By John Corſe, Eſq. Communicated by the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—Some obſervations on the ſtructure of the teeth of graminivorous quadrupeds, particularly thoſe of the elephant and ſus Æthiopicus. By Everard Home, Eſq. F. R. S.—Experiments to determine the quantity of tanning principle and gallic acid contained in the bark of various trees. By George Biggin, Eſq. Communicated by the Right Hon. Sir Joſeph Banks, K. B. P. R. S.—Eſſay on the reſolution of Algebraic equations; attempting to diſtinguiſh particularly the real principle of every method, and the true cauſes of the limitations to which it is ſubject By Griffin Wilſon, Eſq. Communicated by Edward Whitaker Gray, M. D. Sec. R. S.—On different ſorts of lime uſed in agriculture. By Smithſon Tennant, Eſq. F. R. S.—Experiments and obſervations on ſhell and bone. By Charles Hatchett, Eſq. F. R. S.—A catalogue of Oriental manuſcripts, preſented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Eſq. F. R. S.—Preſents received from November 1798 to June 1799.

On the 7th inſtant the Society met for the firſt time ſince the long vacation, when the Croonian Lecture on muſcular motion, by Everard Home, Eſq. was read. The ſubject was on the conſtruction of the membrana tympani. At the ſame ſitting a picture, by Mr. Brown, of the late Mr. Smeaton, civil engineer, was preſented to the Society by Alexander Aubert, Eſq.*

Nov. 14. The concluſion of the Croonian Lecture was read, and a letter addreſſed to Count Bruhl on the orbit of the comet ſeen in Auguſt.

Nov. 21. A paper by Dr. Herſchel on the power of penetrating into ſpace by means of teleſcopes. This, the Doctor remarks, is connected with ſomething different from their magnifying power. The extreme ſenſibility of the optic

* The meeting-room is hung round with portraits of deceaſed members; that of Sir Iſaac Newton being placed over the preſident's chair.

nerve

nerve is ſuch as to require twenty minutes reſt, on coming from the light, before (when his head is covered with a black hood) he is able to diſcern ſmall teleſcopic ſtars: an equal time is neceſſary after a ſtar of the ſecond or third magnitude paſſes the field of the teleſcope.—The paper was not gone through at this ſitting.

METEORS.

Fiery Meteors have been more than uſually common of late.

On Saturday night, the 2d of November, at half paſt ten o'clock, a meteor, or ball of fire, paſſed through the air immediately over the town of Pocklington, accompanied with a moſt beautiful train of fire reſembling the tail of a rocket, and appearing to be about 15 yards in length. The direction was from the north-eaſt to the ſouth or ſouth-weſt, and continued viſible for about half a minute.

On Tueſday morning the 12th of November, about ſix o'clock, a meteor, or ball of fire, accompanied with a beautiful train, was obſerved in different parts of Staffordſhire. It was preceded by ſeveral flaſhes of vivid lightning.

The ſame, or a ſimilar meteor, was ſeen at the ſame time in Yorkſhire. The following is a deſcription of it, as ſeen at Hull and the neighbourhood:—On the morning of the 12th of November, between the hours of five and ſix, the heavens exhibited an awfully grand appearance. The ſetting moon became partially obſcured by dark cloudy ſpots or ſtreaks: in oppoſition to her was ſeen a lunar rainbow of the moſt beautifully varied colours; after which, the middle region of the air was illuminated by meteors croſſing each other in different directions, and leaving behind them long ſparkling trains, which were viſible for two or three minutes after theſe luminous bodies had diſappeared; one of theſe meteors, more brilliant than the reſt, illuminated the whole firmament, and, by its apparent approximation to the earth, created ſome alarm. The thermometer was that morning at 50 degrees. The air, which the preceding night was cold and froſty, became remarkably cloſe and warm, and produced on the walls

walls and furniture in houſes an unuſual dampneſs and humidity.

CONVERSION OF IRON INTO STEEL.

In our laſt we noticed that Mr. Muſhet, in varying the experiments ſuggeſted by C. Clouet's proceſs for the converſion of iron into ſteel by cementation with carbonat of lime, had found that when lime, *previouſly deprived of its carbonic acid*, was uſed with the iron, the reſult was, notwithſtanding, caſt ſteel; and that, therefore, he found himſelf obliged to reject the idea of the carbon neceſſary to the converſion of iron into ſteel, having been furniſhed by the decompoſition of carbonic acid.

We alſo in our laſt Number laid before our readers C. Guyton's report of the converſion of iron into caſt ſteel by means of the diamond, which is generally held to be pure carbon, and which, having diſappeared in the proceſs, was therefore believed to have entered into chemical union with the iron by the affinity exerted between them by means of an high temperature.

From the tenor of the letter from Mr. Muſhet, which furniſhed us with matter for the notice firſt above referred to, we were led to ſuſpect that, however probable it might be that in the caſe laſt mentioned the carbon, which went to ſteelify the iron, was furniſhed by the diamond; yet, as Mr. Muſhet's experiments went to ſhew that carbon could find its way from the ignited gas of the furnace to the iron, through materials which, on firſt view, one would hardly think pervious to ſuch a principle, it was certainly poſſible that the French chemiſts might be miſtaken, and that the carbon, which converted the iron, might not after all have been furniſhed by the diamond.

Since, then, we correſponded with Mr. Muſhet on the ſubject, and propoſed that the experiment made at the Polytechnic School ſhould be repeated, only *keeping out the diamond*. As the idea aroſe from, and might indeed be ſaid to be embraced in, the facts preſented by Mr. Muſhet; and as the proceſs coincided ſo nearly with others which he had communicated as executed and in train, we knew no perſon

ſo likely to do it complete juſtice, or ſo well qualified by great experience to guard againſt error. In this we have not been deceived. Mr. Muſhet, whoſe zeal in every thing that concerns the improvement of the Britiſh iron manufactory entitles him to the gratitude of his country, has proved, by ſeveral concluſive experiments, that the French chemiſts would have had ſteel from their experiment even if no diamond had been employed in it. We ſhall not detain our readers longer, but content ourſelves with laying before them

An Account of the Experiments made by Mr. Muſhet with a view to prove whether the Experiment made at the Polytechnic School reſpecting the Converſion of Iron into Steel by means of the Diamond is concluſive.

I. "I introduced into a crucible ſome pieces of ſoft malleable iron weighing 1250 grains. A larger crucible was next taken, half filled with ſand pounded from the fire-ſtone of which the blaſt-furnace hearths at Clyde are made. The ſmall crucible containing the iron was inverted upon the ſurface of the ſand in the ſecond crucible, and forced ſo far into it as to bring the iron and ſand nearly into contact. The ſpace betwixt the exterior ſurface of the ſmall crucible, and the interior ſurface of the large one, was completely filled with the ſame ſand; the bottom of the inverted one alſo received half an inch of ſand. This quite filled the outer crucible, and made it have the appearance of being entirely filled with ſand: a well fitted fire-clay cover ſealed the whole. After a violent heat for 65 minutes the crucibles were withdrawn from the furnace: when cool, I found a ſlight crack in the exterior one; the ſand however remained entire, and firmly cemented together, though not vitrified, forming a thick porous lute ſurrounding the ſmall crucible. Upon removing the ſand, and examining the interior veſſel, I found the pieces of iron fuſed into one ſolid maſs, which proved ſoft ſteel. The maſs of ſand, which occupied the mouth of the inverted pot, contained ſeveral detached globules of ſteel; theſe, during the intenſity of the heat, and the extreme diviſion of the fluid, had penetrated the ſand, and one of them had actually reached the bottom of the outer crucible. Theſe maſſes

had in their deſcent conveyed a blackiſh-blue colour to the ſand, and the fuſion of the whole had glazed the bottom and ſides of the inverted crucible, unoccupied by the ſand, with the ſame colour. The principle button, and ſmall pieces, weighed 1229 grains: loſs, 21 grains, $= \frac{1}{70}$ of the original weight of the iron.

"The quality of this ſteel was uncommonly red-ſhort, and it was with difficulty it would take a form under the hammer. When cold it was much tougher than good ſteel is ever found to be, and diſtended in this ſtate aſtoniſhingly under the hammer. The fuſion, however, had been moſt complete; every part of the reſult poſſeſſed a cryſtalliſed ſurface more or leſs accurate. Upon the whole, the quality of this ſteel is, I think, ſimilar to ſome deſcribed lately by C. Clouet. Its uncommon ſoftneſs, while it poſſeſſed not the property of drawing into ſhape with a ſmooth uncracked ſurface, led me to conceive that the fuſion was urged too ſpeedily, and before the ſufficient quantity of carbon had been taken up which conſtitutes good ſteel. This induced me to repeat the experiment in the following manner:—

II. "Having prepared, in an annealing-furnace, three crucibles, made from Sturbridge clay, of various ſizes; into the ſmalleſt one I put five pieces of malleable iron weighing 1875 grains; upon the top of which I wedged a high-baked cover of fire-clay: upon the top of this cover, and to a level with the edges of the crucible, I introduced infuſible ſand. This operation was performed upon a plate of red-hot iron, leſt the ſudden contact of the cold air ſhould endanger the ſoundneſs of the pots. The ſmall crucible was next introduced into the ſecond ſize, and a cover fitted over the two. Laſtly, this double crucible was placed into one ſo much larger as to admit of a ſtratum of ſand, nearly half an inch thick, in ſuch a manner as to inſulate the former moſt completely. This operation finiſhed by covering the outſide crucible with a large fire-clay cover. My reaſon for leaving the vacant ſpace betwixt the two innermoſt crucibles not filled up was, to guard againſt any error in the reſult, which might probably be occaſioned by the action of the interior fire-clay cover, and the ſand, in their reſpective degrees of

ſhrinkage and expanſion. The whole was expoſed to a moderate white heat for nearly an hour, and for forty minutes further a heat equal to the whole power of the furnace. The crucibles were ſafely withdrawn, and, when examined, the following facts preſented themſelves:—

"The outer crucible remained entire; the ſand, next, formed an entire veſſel conſiderably connected. The ſecond crucible was alſo found, but in the bottom of it I found an irregular metallic maſs. A ſmall hole in the ſide of the interior pot had allowed nearly one-half of the original weight of iron to paſs; the remainder of it, forming a very beautiful circular button, reſted upon the bottom. The ſurface was cryſtalliſed in radii, ſhooting from a central point upon the upper ſurface of the button. The collective weight was 1858 grains: loſs, 17 grains, $= \frac{1}{110}$ part the weight of the iron. When the metal was lodged in the ſmall crucible, the ſides and bottom were glazed, as in the former experiment.

III. "The ſame experiment was repeated with bottle glaſs, and a ſimilar length and degree of heat applied. The reduction of the glaſs was entire. Betwixt the exterior and middle crucible, the glaſs appeared as an entire interpoſed veſſel, completely ſurrounding the two ſmaller ones. Upon examining the interior crucible, I found that the contact of the bottle-glaſs had fuſed the cover of fire-clay fitted over the iron, and that the promiſcuous fuſion of the glaſs and clay formed a very fine dark-green tranſparent maſs, beneath which lodged the metallic button. The upper ſurface ſtill preſerved the acuteneſs of the original angles upon one piece of iron; the others had reſolved themſelves by fuſion into a very fine ingot of ſoft ſteel with partially cryſtalliſed edges. The weight of the whole was 1759 grains; leſs than when put in, five grains; equal to $\frac{1}{3}\frac{1}{5}\frac{4}{1}$ part of the whole.—I am convinced that by prolonging the heat ten minutes, the whole iron would have entered into fuſion. I would from this and other experiments infer, that the iron is of more difficult carbonation when a fuſed medium is made uſe of, than when either pure ſand, pure lime, or clay, are uſed.

IV. "In order that no proof might be further wanting to eſtabliſh the fact of the iron receiving the carbon from its ſo-

lution in caloric, and to obviate any objection which might probably be ſtarted againſt this, by ſuppoſing that the carbon might be conveyed through the porous medium of the earths, or the bottle-glaſs, *before* it entered into fuſion, I made the following experiment:—Having made a mould in ſand, I poured into it thinly fuſed calcareous ſtone; and, while the fluid was thin, introduced a ſmall rod of red-hot iron, ſuſpended by a very ſmall braſs wire. The dimenſions of the rod were ſo proportioned to the mould that 5-8ths of an inch of a viril cruſt encircled the iron on all ſides. The wire became fuſed and diſengaged as the fluid was conſolidating, ſo that every avenue to the iron was completely ſhut up. The maſs was carefully cooled to prevent ſhivering, and as carefully heated, when introduced into the furnace, in a crucible inverted in one larger, and the vacancy betwixt each ſhut up from air by means of pounded bottle-glaſs. A cover was fitted on, and a violent heat urged for nearly two hours. The crucibles were taken out, free from blemiſh, and cemented as one compact maſs. When cool, I found the metal in the interior crucible reſolved into a very fine ingot, which proved afterwards to be ſoft ſteel; capable, however, of hammering and hardening to great advantage. The weight of ſteel obtained was 1157 grains; loſt, 33 grains, $= \frac{1}{36}$ part of the whole. The fuſed lime, which had formerly cooled of a pure whitiſh porcelain colour variegated with blue ſtreaks, was now of a ſpongy brown colour, exactly reſembling the lava of the blaſt-furnace when oxygenated crude iron is produced. This alteration was unqueſtionably owing to the deficient weight in metal having in part been oxydated by the interior air of the crucible, or by oxygen from the furnace, and united to the earth in the ſtate of a fuſed oxyde. The great difference in the loſs of metal ſuſtained in theſe two laſt experiments muſt be ſought for in the quantity of iron taken up by the flux: in the others, the ſolidity and tranſparency of the glaſs were uncommonly fine; in theſe the flux was opaque, dark, and porous, evidently ſurcharged with iron.

"It will appear from theſe experiments, that we are ſtill without any ſatisfactory or concluſive proof of the ſteelification of iron ſolely by means of the diamond.

"There

"There is one experiment which would, if ſucceſsful, eſtabliſh the point. C. Clouet calculates, that, to conſtitute grey crude iron, 1-6th of carbon is neceſſary. Were ſix parts of iron, and a diamond, equal to 1-6th of this, ſubjected to the ſame proceſs as formerly, and the reſult *grey*, or *carbonated crude iron*, inſtead of ſteel, the inference would be juſt, and the concluſion ſatisfactory.

"In the preſent ſtate of my information, I doubt whether the diamond afforded even one particle of carbon to the iron. From many experiments, wherein I uſed very pure well charred wood and malleable iron, I uniformly found that the weight of the ſteeliſied ingot nearly equalled the ſum of that of the iron and carbon originally introduced: ſo that, in place of having a loſs *of real iron* equal to $\frac{1}{4^{\frac{1}{0}}}$, as experienced by the Pariſian chemiſts, had the diamond been taken up by the iron, the ingot of ſteel would have weighed within $\frac{1}{2^{\frac{1}{0}}}$ part of the aggregate weight of both, and from $\frac{1}{6^{\frac{1}{0}}}$ to $\frac{1}{7^{\frac{1}{0}}}$ part more than the iron at firſt introduced.

"So far as I have gone, I think my experiments quite concluſive: the others you mention ſhall be attended to in due time."

Monument to Count Rumford.

In the introduction to a German tranſlation of Count Rumford's Eſſays, lately publiſhed at Weimar, we find, among other anecdotes relative to the author, an account of a monument that was erected at Munich in the year 1795 in commemoration of his public ſervices.

This monument, which is ſituated in a beautiful public garden adjoining to the ramparts of the city, was erected (without the knowledge of the Count, while he was abſent from Bavaria,) by the principal inhabitants of Munich, with the permiſſion and approbation of the late Elector.

The monument is a ſolid pile of a quadrangular form, conſtructed of hewn ſtone, about twenty feet in height; and it has two principal fronts, which are ornamented with ſculpture and inſcriptions.

Upon one of theſe fronts there is a baſſo relievo of two figures

figures at whole length repreſenting the Genius of Plenty leading Bavaria by the hand, and ſtrewing her path with flowers. Under this emblematical piece of ſculpture, upon a broad tablet of Bavarian marble, there is the following inſcription in the German language:—

LUSTWANDLER STEH!
DANK STÆRKET DEN GENUS.
EIN SCHŒPFERISCHER WINK CARL THEODORES,
VON MENSCHENFREUNDE RUMFORD,
MIT GEIST. GEFIHL, UND LIEB' GEFAST,
HAT DIESE EINMALS ŒDE GEGEND
IN DAS WAS DU NUN UM DICH SIHEST
VEREDELT.

Which may be thus tranſlated:

Stay, Rambler!
Thankfulneſs increaſes Enjoyment.
The creative glance of Charles Theodore
Rumford, the Friend of Mankind,
With Genius, Taſte, and Love inſpired,
Changed this once deſert place
Into what thou now beholdeſt.

Upon the oppoſite front of the monument there is a medallion of Count Rumford, as large as the life, reckoned a good likeneſs, and under it the following inſcription:—

IHM
DER DAS SCHMAEHLICHSTE OEFFENTLICHER UEBEL,
DEN MUESSIGANG UND BETTEL TILGTE;
DER ARMUTH HILF, GEWERB UND SITTEN,
DER VATERLAENDISCHEN IUGEND
SO MANCHE BILDUNGSANSTALT GAB:
LUSTWANDLER GEH!
UND SINNE NACH IHM GLEICH ZU SEIN
AN GEIST UND THAT,
UND UNS
AN DANK.

In

In Engliſh as follows :—

To Him
Who rooted out the moſt diſgraceful of public Evils,
Idleneſs and Mendicity;
Who relieved and inſtructed the Poor,
And founded many Inſtitutions
For the Education of our Youth;
Go, Wanderer!
Strive to equal him
In Genius and Activity,
And us
In Gratitude!

That Count Rumford may long live to enjoy the heart-felt ſatisfaction that muſt reſult from the contemplation of the ſucceſs of his labours in promoting the happineſs of mankind, muſt be the ſincere wiſh of every lover of ſcience, and of every true friend to virtue and morality.

LUMINOUS APPEARANCE PRODUCED BY SUGAR.

In the proceſs of breaking fine white ſugar in a dark apartment, a very perceptible luminous appearance is obſerved, the cauſe of which is as much unknown to us as that produced by rubbing againſt each other two flints, particularly thoſe of Iceland. The following is offered by Dr. Juch*, of Würzburg, as a kind of explanation of this phenomenon:

"While breaking refined ſugar," ſays he, "in ſmall pieces with a hammer and blunt knife, I ſuddenly perceived a very ſingular ſmell, which had a great reſemblance to that of fuming nitrous acid. I looked immediately round me to ſee whether ſome veſſel containing that acid might not be in the neighbourhood; but I ſaw none, and was certain that nothing of the kind had been in the room. I then examined the circumſtance more narrowly, and found, on breaking fine ſugar, that a ſmell ſimilar to that of aquafortis was always perceived by every perſon preſent.

"This ſimultaneous diſengagement of light, and of the ſmell of nitrous acid, I explain in the following manner:—When

* In *Scherer's Journal der Chemie*, No. 10.

ſugar

ſugar is broken, a new ſurface being preſented to the air, a real acidifying proceſs inſtantly takes place: a ſmall portion of the oxygen of the atmoſphere unites itſelf with the ſugar, but not enough to form ſaccharine acid, as the quantity is not perhaps ſufficient for that purpoſe; and at the ſame moment the atmoſphere in contact with the new ſurface of the ſugar is ſo changed, in regard to the proportion of its component parts, that it affects the olfactory organs in the ſame manner as nitrous acid. This phenomenon may receive ſome farther illuſtration by repeating the experiment in a cloſe veſſel."

A SINGULAR OXYDATION OF IRON.

Dr. Juch alſo gives the following ſingular notice:—

"I had," ſays he, "a ſmall leather flaſk filled with iron filings, which I frequently uſed with my electrophorus both charged and uncharged. I had occaſion one day for a quantity of pure iron, and, having no other at hand, had recourſe to the filings in the flaſk, which I knew were pure. I emptied it on a piece of paper. I laid the paper on my hand in order to convey it to the place of its deſtination; but I had ſcarcely held it a few ſeconds when I perceived a ſtrong heat, which increaſed ſo much that I could no longer hold the paper. Some minutes after, the paper became brown, and at length took fire. It did not, however, flame. The iron filings were in a ſtate of ignition; and when the heat had decreaſed, I found the iron converted into a highly friable oxyd."

ELECTRICITY OF ICE.

The ſame author mentions, that electric ſparks may be obtained by the uſual manipulation from a cylinder of ice. Thoſe who may not be afraid of the labour, can make the experiment at the temperature of $-16\frac{1}{2}$ R. $= -5\cdot125$ Fahr.

THE

PHILOSOPHICAL MAGAZINE.

DECEMBER 1799.

I. *On different Sorts of Lime used in Agriculture.* By SMITHSON TENNANT, *Esq. F.R.S.* *

I WAS informed last summer, that, in the neighbourhood of Doncaster, two kinds of lime were employed in agriculture, which were supposed to differ materially in their effects. One of these, which was procured near the town, it was necessary to use sparingly, and to spread very evenly over the land: for it was said, that a large proportion of it, instead of increasing, diminished the fertility of the soil; and that, wherever a heap of it was left in one spot, all vegetation was prevented for many years. Fifty or sixty bushels upon an acre, were considered to be as much as could be used with advantage. The other sort of lime, which was obtained from a village near Ferry-bridge, though considerably dearer from the distant carriage, was more frequently employed, on account of its superior utility. A large quantity was never found to be injurious; and the spots which were entirely covered with it, instead of being rendered barren, became remarkably fertile. The different properties ascribed to these two kinds of lime were so very distinct, that it seemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of ascertaining them more fully, and

* From the *Phil. Trans. of the Royal Society for* 1799, Part II.

of attempting to diſcover the nature of the ingredients from whence the difference aroſe. For this purpoſe I procured ſome pieces of each ſort of limeſtone, and firſt tried what would be their effect upon vegetables, in their natural ſtate, by reducing them to coarſe powder, and ſowing in them the ſeeds of different plants. In both kinds the ſeeds grew equally well, and nearly in the ſame manner as they would in ſand, or any other ſubſtance which affords no nouriſhment to vegetables. Pieces of each ſort of ſtone were then burnt to lime; and, after they had been expoſed for ſome weeks to the air, that their cauſticity might be diminiſhed, ſome ſeeds were ſown in them. In the kind of lime which was found moſt beneficial to land, almoſt all the ſeeds came up, and continued to grow as long as they were ſupplied with water; and the roots of the plants had many fibres, which had penetrated to the bottom of the cup in which they grew. Upon examining the compoſition of this ſort of lime, it proved to conſiſt entirely of calcareous earth. By its expoſure to the air for about three months, it was found to have abſorbed 4-5ths of the fixed air required to ſaturate it. In the other kind, a few only of the ſeeds grew, and the plants produced from them had hardly any ſtalks or roots, being formed almoſt entirely of the two ſeed-leaves, which lay quite looſe upon the ſurface. This ſort of lime, being ſpread upon a garden ſoil, to the thickneſs of about the tenth of an inch, prevented nearly all the ſeeds which had been ſown from coming up, whilſt no injury was occaſioned by common lime uſed in the ſame manner. Upon examining the compoſition of this ſubſtance, which was ſo deſtructive to the plants, it was diſcovered to contain three parts of pure calcareous earth, and two of magneſia. The quantity of fixed air which it had abſorbed, by being expoſed for about the ſame time as the pure lime juſt mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it ſeemed probable that the magneſia contained in this lime was the cauſe of its peculiar properties, the following experiments were made, to determine the effects of that ſubſtance upon the growth of vegetables. Some ſeeds, chiefly of colewort, which were preferred from their growing quickly,

were

were ſown in uncalcined magneſia; but, though they ſprouted, the leaves never roſe above the ſurface, and the plants were entirely without roots, nor did they appear to grow better in magneſia which had been waſhed in water containing fixed air. Calcined magneſia was, however, much more deſtructive, as the ſeeds would not come up in it. To compare its effects on vegetables with thoſe of lime, each of theſe earths was mixed, in different proportions, with ſand, in ſmall cups, in which ſeeds were then ſown. The lime was obtained from marble; and, before it was put into the ſand, was made to fall to powder, by being moiſtened with water. In a mixture of four ounces of ſand with three or four grains of calcined magneſia, it was a long time before the ſeeds came up, and the plants had hardly any roots or ſtalks; and, with ten grains or more of magneſia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the ſeeds more than three or four of magneſia, and the injurious effects were not ſo laſting. The lime, by abſorbing fixed air, ſoon loſt its deſtructive properties; ſo that, after keeping theſe mixtures four or five weeks, ſeeds were found to grow in that with forty grains of lime, nearly as well as in pure ſand; but, in that with four grains of magneſia, they produced only the ſeed-leaves, as was deſcribed before. It was neceſſary occaſionally to break in pieces the ſand which had ſo much lime, as it would otherwiſe have been too hard to admit the ſeeds to penetrate through it. Plants will bear a much larger proportion of magneſia in vegetable ſoil than in ſand: with twenty grains, however, of calcined magneſia, in as much ſoil as was equal in bulk to four ounces of ſand, the ſeeds produced only the ſeed-leaves without roots; and, with about forty grains, they were entirely prevented from coming up.

In countries where the magneſian lime is employed, it was ſaid, that the barrenneſs of any ſpot on which a heap of it had been laid, would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured ſome pieces of mortar, made of this ſpecies of lime, from two houſes, one of which had been built three, and the other eight years: they were taken from the outſide

of the building, where they had been expoſed to the air. After they were reduced to powder, ſeeds were ſown in them. Only a few came up, and even thoſe produced merely the ſeed-leaves, without any roots. As plants would grow in the limeſtone from which this ſpecies of lime was formed, although not in the mortar made from it, I wiſhed to know what proportion of the fixed air, originally contained in the limeſtone, had been abſorbed by the mortar. For this purpoſe a piece of it was finely powdered, to render it of an uniform quality: it was then tried how much of this powder, and of the limeſtone, would ſaturate the ſame quantity of acid: by this means I aſcertained the proportions of limeſtone and mortar, containing equal quantities of the magneſian lime. The fixed air being obtained from them in thoſe proportions, and meaſured in an inverted veſſel, with quickſilver, it was found that the mortar which had been expoſed three years had abſorbed 43, and that of eight years only 47 hundredths of the quantity originally contained in the limeſtone. I was not able to obtain any mortar which had been made earlier, though it might deſerve to be known how much fixed air it was ultimately capable of abſorbing. Common mortar, which had been expoſed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps under circumſtances more favourable to vegetation, the ſame quantity of magneſia would not be equally pernicious.

Magneſian limeſtone may be eaſily diſtinguiſhed from that which is purely calcareous by the ſlowneſs of its ſolution in acids, which is ſo conſiderable, that even the ſofteſt kind of the former is much longer in diſſolving than marble. From this property of the magneſian limeſtone, there appeared to be reaſon for ſuſpecting that the kind of marble which had been called Dolomite, from M. Dolomieu, who firſt remarked its peculiarity in diſſolving ſlowly, might alſo be ſimilar in its compoſition. An analyſis of this ſubſtance was lately given in the *Journal de Phyſique*: but this is probably erroneous; for, upon examining three ſpecimens, they

were

were found to conſiſt of magneſia and calcareous earth, like the magneſian limeſtone; ſo that it ought no doubt to be conſidered as the ſame ſpecies of ſtone, but in a ſtate of greater purity. The pieces of dolomite were from different places; one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many ſtatues of Grecian workmanſhip are made of it, and no quarries of a ſimilar kind are known in Italy; the ſecond was ſaid to have been thrown up by Mount Veſuvius; and the third was from Iona, one of the weſtern iſlands of Scotland. In many kinds of common marble, ſmall particles and veins may be obſerved, which are a long time in diſſolving. Theſe, upon examination, I diſcovered to contain a conſiderable proportion of magneſia; but, as they were probably not quite free from the ſurrounding marble, I did not aſcertain the quantity preciſely.

The cryſtalliſed ſtructure which may generally be obſerved in the magneſian limeſtone, ſeems to ſhew that it has not been formed by the accidental union of the two earths, but muſt have reſulted from their chemical combination. The difficulty of diſſolving it, may alſo ariſe from the attraction of the different component parts to each other. The mortar formed from this kind of lime is as ſoluble in acids as common marble; and the ſubſtances of which it conſiſts are eaſily ſeparated. The magneſia may be taken from it by boiling it in muriated lime, and lime is precipitated by it from lime water; but neither of theſe effects can be produced by the ſtone before it is calcined.

Magneſian limeſtone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles, from a little ſouth-weſt of Worksop in Nottinghamſhire, to near Ferry-bridge in Yorkſhire. About five or ſix miles further north there is a quarry of it, near Sherburn; but whether this is a continuation from the ſtratum near Ferry-bridge, I have not learnt. From ſome ſpecimens which were ſent me, I find that the cathedral and walls of York are made of it. I have not been able to learn whether there were any ſhells in the limeſtone of the tract of country before-mentioned. In Mr. Marſhall's account of the agriculture of the midland counties, he ſpeaks of the lime made at Breedon, near Derby, as deſtructive to vegetables, when uſed

in

in large quantities. I therefore procured some pieces of it, and they were discovered to contain nearly the same proportion of magnesia as that before described. In this quarry the stone is frequently crystallised in a rhomboidal form; and petrified shells, not calcareous, but similar in composition to the stone itself, are sometimes, but very rarely, found in it. This substance seems to be common in Northumberland. In the third volume of the Annals of Agriculture, Dr. Fenwick, of Newcastle, observes, that the farmers of that country divide limes into hot and mild. The former of these is no doubt magnesian, as it has similar effects on the soil; and he remarks, that it is not so easily dissolved in acids as the latter. At Matlock, in Derbyshire, the two kinds are contiguous to each other; the rocks on the side of the river where the houses are built being magnesian, and on the other calcareous. The magnesian rock appears also to be incumbent upon a calcareous stratum; for, in descending a cave formed in this rock, a distinct vein of common limestone may be observed, which contains no magnesia. The latter stratum is very full of shells; but, though there are some also in the magnesian rock, yet they are very rare. In the following tables, containing the analysis of various specimens, some other places are mentioned where this substance is found, but of which I received no further information.

After it was known that the magnesian marble and limestone consisted of the two earths, their proportion was attempted to be discovered, by trying how much gypsum and Epsom salt could be obtained, by means of vitriolic acid, from a certain weight of each specimen. When the superfluous vitriolic acid had been evaporated by heat, the Epsom salt was separated from the gypsum by water. The result of these trials is expressed in the following table:—

	Dry Gypsum.	Dry Epsom salt.
5 grains of limestone from Breedon gave	3·9	3·15
——————— Matlock -	3·95	2·9
——————— Worksop -	3·8	3·0
——————— York -	3·8	3·1
3 grains of calcareous spar, and 1 grain of calcined magnesia, gave -	3·9	2·7

As the preceding method of eſtimating the quantities of magneſia and calcareous earth is liable to conſiderable error, I afterwards examined them in the following manner, which ſeems capable of great exactneſs:—Twenty-five grains of each ſubſtance were diſſolved, by marine acid, in a cup of platina, and, after the ſolution was evaporated to dryneſs, it was made red-hot for a few minutes. The maſs remaining in the cup, which conſiſted of muriated lime, and of the magneſia freed from the acid, was waſhed out with water, and poured into a phial. There was then added to it a known quantity of diluted marine acid, ſomewhat more than was ſufficient to redilſolve the magneſia, and, after the ſolution, a certain weight of calcareous ſpar, part of which would be diſſolved by the ſuperfluous acid. By the quantity of ſpar remaining undiſſolved, it was learnt how much acid was required to diſſolve the magneſia. The iron and argillaceous earth contained in ſome ſpecimens, were precipitated by the ſpar, and therefore could not occaſion any error. The calcareous ſpar, however, diſſolved more ſlowly where there was argillaceous earth, as it became coated with it; but this incruſtation was occaſionally removed, and, in all the experiments, the ſpar was left in the ſolution till it ſuffered no further diminution. For this purpoſe it was neceſſary to keep them ſlightly warm for ſome days, during which time the phials were generally cloſed, to prevent any eſcape of the acid.

The firſt experiment in the following table was made upon known quantities of magneſia and calcareous earth, to try the accuracy of the proceſs. For this purpoſe, alſo, the ſecond was repeated upon a piece of limeſtone, previouſly powdered, to render every part of it of the ſame quality. The firſt column ſhews the quantity of calcareous ſpar which might have been diſſolved by the acid required to take up the magneſia. The ſecond ſhews the correſponding quantities of magneſia in 25 grains of each ſubſtance. The third expreſſes the quantity of lime. This was inferred by ſubtracting the weight of the magneſia, and of the iron and clay, from 13.2 grains, the weight of the whole quantity of earth in 25 grains of limeſtone. This is probably not very incorrect,

incorrect, as, in two ſpecimens which differed moſt in the proportion of magneſia and lime, the weight of the two earths was nearly the ſame.

A piece of dolomite, from Rome, was wrapped in a thin leaf of platina, that no part of it might be loſt, and, being then expoſed to a ſtrong heat, left of earth - 52.9 *per cent.*

Dolomite from Mount Veſuvius - 52.8

Breedon limeſtone - - 52.4

Calcareous ſpar left of lime - - 55.8

In three of the experiments, alſo, the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to diſſolve it, it correſponded very nearly with that put down.

A quantity of marine acid which would diſſolve 15 grains of calcareous ſpar, would alſo diſſolve 5.5 of calcined magneſia, and 2.5 grains of ſpar; ſo that 12.5 grains of ſpar, required the ſame quantity of acid as 5.5 grains of magneſia.

The magneſia uſed was very pure, and made red-hot immediately before it was weighed.

Subſtances examined.	Quantity of ſpar which the acid, required to take up the magneſia, would have diſſolved.	Quantity of magneſia.	Quantity of lime.	Iron and clay.
Mixture of 5.5 grains of magneſia and 14 grains of calcareous ſpar - - - -	12.5	5.5	7.8	0
25 grains of Breedon limeſtone, previouſly powdered - -	11.53	5.071	7.929	.2
25 grains from part of the ſame powder - - - -	11.56	5.082	7.913	.2
25 grains of dolomite from Rome - - - -	12.2	5.37	7.73	.1
——— dolomite from Iona - - - -	10.1	4.4	7.8	1.0 Inſoluble ſubſtance
——— Veſuvian dolomite - - - -	10.38	4.565	8.575	.06
A ſecond experiment, from part of the ſame Veſuvian dolomite - - -	10.03	4.411	8.849	.06
25 grains of magneſian limeſtone from Wantworth, near Doncaſter - - -	12.75	5.61	7.34	.25
——— Thorpe arch -	10.95	4.84	7.8	.6
——— Matlock - -	12.5	5.5	7.388	.31
——— York-minſter -	11.	4.84	8.26	.1
——— Worksop - -	11.6	5.104	7.496	.6
——— Sherburn - -	11.5	5.08	7.56	.56
——— Weſtminſter-hall	10.1	4.44	8.37	.4

II. *Agenda,*

II. *Agenda, or a Collection of Obſervations and Reſearches, the Reſults of which may ſerve as the Foundation for a Theory of the Earth.* By M. De Sausſure.

[Concluded from Page 140.]

CHAP. XXIII.

Inſtruments neceſſary for the Geological Traveller.

1. THE moſt neceſſary inſtrument is the miner's hammer. It will be requiſite to have two, of different ſizes: one ſmall, to break ſmall fragments of rolled pebbles, by holding them in the left hand while you ſtrike with the right. Its weight, including the handle, ought to be about ten ounces. The other muſt be larger, to detach fragments of rock, and to break large pebbles: its weight ought to be quadruple that of the ſmall one. When I travel on horſeback, I have theſe two hammers ſuſpended from the bow of my ſaddle.

1. A. Two ſtone-cutter's chiſels: one ſmall, of from a line to a line and a half, to detach ſmall cryſtals, or other objects of ſmall bulk; the other, ſeven or eight lines.

2. To try the hardneſs of foſſils, a piece of ſteel to ſtrike fire will be neceſſary; alſo a triangular file, pretty fine, and a ſtrong bodkin of tempered ſteel.

3. Nitrous acid, with M. De Morveau's boxes of reagents.

3. A. An artificial magnet, in a caſe, with a ſteel pivot on which it can be placed, to try the magnetiſm of foſſils.

4. A magnifying glaſs of three inches focus, in order to enable the obſerver to form a general idea of any foſſil: another, of an inch focus, to examine its ſeparated parts; and one of five or ſix lines for cloſer examination. Theſe three magnifiers muſt be always in the traveller's pocket, or ready at hand: but, beſides theſe, he muſt have, for his cloſet at home, a microſcope furniſhed with a micrometer.

5. Teleſcopes, to obſerve inacceſſible mines and diſtant mountains.

6. A pocket portfolio, with prepared paper for writing on,

with a pencil of tin ſolder, which it is not neceſſary always to cut, and the writing of which is not ſo eaſily effaced as that of plumbago. In this portfolio the traveller muſt write out, on the ſpot, the ſketch of his journal, and inſert ſuch obſervations as occur to him; but he muſt take the trouble to tranſcribe theſe notes at more length, preſerving the primitive notes, which will always retain a character of truth, and for that reaſon people are fond of recurring to them.

7. Some quires of brown paper, a few ſheets of which may be carried in the pocket for wrapping up on the ſpot ſpecimens of the ſtones you collect, the characters of which ought to be marked on the cover. You may afterwards pack them with hay into a bag deſtined for that purpoſe, until you have a ſufficient quantity to form a box, which you may ſend home by the public carriages wherever you find an oportunity; but, as it is fatiguing for the traveller to load his pockets during the time of his excurſions, and as the guides often loſe them on purpoſe in order to get rid of them, I have behind my ſaddle two leathern bags, into which I put them till I come to ſome halting-place, where I have time to pack them with hay into a bag. M. Beſſon recommends to thoſe who undertake ſea voyages to write with China ink the characters which ought to accompany minerals in long paſſages, becauſe common ink may be effaced by accidents.

8. A blow-pipe, with its apparatus. As I make much uſe of this inſtrument, which at length fatigues me, though I can blow with my cheeks without exerciſing my breaſt, I cauſed to be conſtructed a pair of portable double bellows, the ſides of which contain each ſixty-two ſquare inches. Theſe bellows can be ſuſpended from the edge of a table; and I put them in motion by preſſing together, between my knees, the two handles, which afterwards ſeparate by the action of the ſpring. This apparatus may be eaſily carried, and is very convenient.

9. A graduated ſemicircle traced out on a copper-plate of a form exactly rectangular, with a plummet ſuſpended from the centre of the ſemicircle. This ſemicircle is the moſt convenient inſtrument for meaſuring the inclination of ſtrata, of veins and declivities; and it may always be carried in a pocket of the portfolio.

10. A

10. A compafs, furnifhed with a crofs ftaff, to find the direction of mountains, chains, vallies, and ftrata.

11. Portable barometers with two mercurial thermometers; one affixed to a barometer to eftimate the temperature of the mercury in the latter, and the other with a bare bulb for meafuring the temperature of the air. Thofe who ftudy meteorology, as well as geology, ought to be furnifhed alfo with an hygrometer and an electrometer.

12. For afcertaining the temperature of the fea, at great depths, it will be neceffary to have a thermometer conftructed like that deferibed in my Travels through the Alps*: for lakes, an apparatus like that pointed out in the note of Section 1399, will be fufficient.

13. Thofe who underftand a little geometry, ought to provide themfelves with a fextant, having an artificial horizon, and alfo a chain, in order that they may be able to meafure a bafe, and thus take the altitude of an inacceffible peak, the breadth of a river, &c. &c. With this fextant they may alfo find the latitudes. In regard to the longitudes, they require, befides inftruments, an expertnefs in this kind of obfervation, which cannot be attained but by mariners or profeffed aftronomers.

14. It will be neceffary alfo to have within reach tools for repairing an inftrument in cafe it fhould happen to be deranged; fuch as pincers, files, turnfcrews, compaffes, gimblets, wire, needles, thread, and packthread.

15. Laftly, fome good map, pafted on canvas, of the country you propofe to examine; and this map ought frequently to be compared with your itinerary, and the bearings given by your compafs.

16. In regard to the care required for the traveller's perfon, he muft have a light drefs made of cloth, without lining, of a white colour, as well as his hat, that he may be lefs expofed to the heat of the fun's rays; with jackets, fome cool for the warm regions and the vallies, and the other warm for the cool regions and eminences; a good great coat; green fpectacles, and a black crape, to fecure the eyes and face from the fnow. Laftly, if he is to pafs the night in the open air, a

* Section 1302, Plate 1, fig. 3.

tent or *cannoniere*, a bear's ſkin to ſleep upon, and woollen blankets.

17. A ſolid light walking-pole: mine for the high Alps is a young fir-plant, extremely dry, ſeven feet in length and 18 lines in diameter at the lower end, which is ſhod with iron tapering to a ſtrong point. Theſe dimenſions may appear large, but nothing can be too ſtrong for the ſteep rocks, glaciers, and ſnow, when you are obliged to take your point of ſupport at a diſtance from you, and to reſt the whole weight of your body on your pole, by holding it in a ſituation very much inclined, and even horizontal, as may be ſeen in the Vignette to the Firſt Volume of my Travels through the Alps. —For mountains which are not ſo ſteep, the traveller may be ſatisfied with poles of leſs ſtrength and ſize; but it will ſtill be neceſſary that they ſhould be four or five feet in height, and ſufficiently ſtrong that a perſon might be able to ſupport himſelf with his two hands by holding them in a horizontal ſituation, according to the attitude of the ſmall figure which is on the left ſide at top of the before-mentioned vignette; for, in traverſing or deſcending a rapid declivity, or in walking on the margin or edge of a precipice, the traveller muſt always ſupport himſelf by his two hands, holding the pole towards the mountain, and not towards the precipice, as thoſe do who have not learned the art of travelling through mountains.

18. To prevent ſlipping on the hard ſnow-ice, and graſs-plats, which are ſtill more dangerous, I would recommend iron cramps, ſuch as thoſe which I have cauſed to be engraved in the third plate of the firſt volume, and which I have long uſed with ſucceſs. In my laſt excurſions, however, I preferred ſhoes, the thick ſoles of which were armed with ſtrong tacks, at the diſtance of eight or nine lines from each other. The heads of theſe tacks are of ſteel, and ſhaped like a ſquare pyramid: I have ſome ſmall ones, the points of which are only two lines and a half in height, and of about the ſame breadth, for the ſnow-rocks and graſs-plats; and others, of double theſe dimenſions, for the hard ſnow.

19. In the laſt place, with regard to proviſions, when the traveller muſt reſide for a conſiderable time in the deſarts, at a diſtance from habitations, and even huts, he may carry with

with him ſome ſalt or pickled meat; but M. Parmentier's ſaloop of potatoes, with cakes of portable ſoup, and bread, will form the moſt nouriſhing food, and what may be contained in the leaſt room. A ſmall iron chaffing-diſh, a ſmall bag filled with charcoal, and a pan of tinned copper or iron, form my kitchen apparatus for the mountains: wooden plates and ſpoons may be found in the remoteſt huts. It will be proper, however, to carry always in the pocket a cup of gum elaſtic, in order that the traveller may at all times eaſily quench his thirſt, a want to which he will be frequently expoſed in his excurſions.

From what has been ſaid, it may be readily ſeen, that the ſtudy of geology will not ſuit the indolent or ſenſual; for the life of the geologue muſt be divided between fatiguing and perilous journies, in which he is deprived of almoſt all the conveniences of life, and the varied and deep reſearches of the cloſet. But what is ſtill more rare, and perhaps more neceſſary than the zeal requiſite to ſurmount theſe obſtacles, is, a mind free from prejudice, filled with an ardent deſire for the truth alone, rather than with a deſire for raiſing or deſtroying ſyſtems; capable of deſcending to minute details indiſpenſibly neceſſary for the correctneſs and certainty of obſervations, and of riſing to grand views and general conceptions. Thoſe fond of ſuch ſtudies, ought not, however, to be diſcouraged by theſe difficulties; there is no traveller who may not make ſome good obſervation, and bring with him at leaſt one ſtone worthy of being employed in the conſtruction of this grand edifice. It is indeed poſſible to be uſeful without attaining to perfection; for I have no doubt that if the mineralogical travels, even the moſt eſteemed, and much more thoſe of the author, be compared with theſe Agenda, there will be found in them many deficiencies, and many obſervations, either imperfect, or even totally forgotten: but I have mentioned the reaſon in the Introduction. Beſides, ſeveral of theſe ideas did not occur till I had finiſhed my travels; and for that reaſon I laboured with more zeal on theſe Agenda, in the hope of rendering young perſons, on their entering this career, capable of performing what coſt me thirty-ſix years of travelling and ſtudy.

III. *On*

III. *On the Nature and Conſtruction of the Sun and Fixed Stars.* By William Herschel, *LL.D. F.R.S.*

[Concluded from Page 123.]

It will now be eaſy to bring the reſult of theſe obſervations into a very narrow compaſs. That the ſun has a very extenſive atmoſphere cannot be doubted; and that this atmoſphere conſiſts of various elaſtic fluids, that are more or leſs lucid and tranſparent, and of which the lucid one is that which furniſhes us with light, ſeems alſo to be fully eſtabliſhed by all the phænomena of its ſpots, of the faculæ, and of the lucid ſurface itſelf. There is no kind of variety in theſe appearances but what may be accounted for with the greateſt facility, from the continual agitation which, we may eaſily conceive, muſt take place in the regions of ſuch extenſive elaſtic fluids.

It will be neceſſary, however, to be a little more particular as to the manner in which I ſuppoſe the lucid fluid of the ſun to be generated in its atmoſphere. An analogy that may be drawn from the generation of clouds in our own atmoſphere, ſeems to be a very proper one, and full of inſtruction. Our clouds are probably decompoſitions of ſome of the elaſtic fluids of the atmoſphere itſelf, when ſuch natural cauſes, as in this grand chemical laboratory are generally at work, act upon them: we may therefore admit, that in the very extenſive atmoſphere of the ſun, from cauſes of the ſame nature, ſimilar phænomena will take place; but with this difference, that the continual and very extenſive decompoſitions of the elaſtic fluids of the ſun are of a phoſphoric nature, and attended with lucid appearances, by giving out light.

If it ſhould be objected, that ſuch violent and unremitting decompoſitions would exhauſt the ſun, we may recur again to our analogy, which will furniſh us with the following reflections. The extent of our own atmoſphere, we ſee, is ſtill preſerved, notwithſtanding the copious decompoſitions of its fluids in clouds and falling rain; in flaſhes of lightning, in meteors, and other luminous phænomena; becauſe there are freſh ſupplies of elaſtic vapours continually aſcending to make

good the waſte occaſioned by thoſe decompoſitions. But it may be urged, that the caſe with the decompoſition of the elaſtic fluids in the ſolar atmoſphere would be very different, ſince light is emitted, and does not return to the ſun, as clouds do to the earth when they deſcend in ſhowers of rain. To which I anſwer, that, in the decompoſition of phoſphoric fluids, every other ingredient but light may alſo return to the body of the ſun. And that the emiſſion of light muſt waſte the ſun, is not a difficulty that can be oppoſed to our hypotheſis: for, as it is an evident fact that the ſun does emit light, the ſame objection, if it could be one, would equally militate againſt every other aſſignable way to account for the phænomenon.

There are, moreover, conſiderations that may leſſen the preſſure of this alledged difficulty. We know the exceeding ſubtilty of light to be ſuch, that in ages of time its emanation from the ſun cannot very ſenſibly leſſen the ſize of this great body. To this may be added, that, very poſſibly, there may alſo be ways of reſtoration to compenſate for what is loſt by the emiſſion of light, though the manner in which this can be brought about ſhould not appear to us. Many of the operations of Nature are carried on in her great laboratory which we cannot comprehend, but now and then we ſee ſome of the tools with which ſhe is at work. We need not wonder that their conſtruction ſhould be ſo ſingular as to induce us to confeſs our ignorance of the method of employing them, but we may reſt aſſured that they are not a mere *luſus naturæ*. I allude to the great number of ſmall teleſcopic comets that have been obſerved, and to the far greater number ſtill that are probably much too ſmall for being noticed by our moſt diligent ſearchers after them. Thoſe ſix, for inſtance, which my ſiſter has diſcovered, I can from examination affirm, had not the leaſt appearance of any ſolid nucleus, and ſeemed to be mere collections of vapours condenſed about a centre. Five more, that I have alſo obſerved, were nearly of the ſame nature. This throws a myſtery over their deſtination, which ſeems to place them in the allegorical view of tools, probably deſigned for ſome ſalutary purpoſes to be wrought by them; and, whether the reſtoration of what is

loſt

loſt to the ſun by the emiſſion of light, the poſſibility of which we have been mentioning above, may not be one of theſe purpoſes, I ſhall not preſume to determine. The motion of the comet, diſcovered by Mr. Meſſier in June 1770, plainly indicated how much its orbit was liable to be changed by the perturbations of the planets; from which, and the little agreement that can be found between the elements of the orbits of all the comets that have been obſerved, it appears clearly that they may be directed to carry their ſalutary influence to any part of the heavens.

My hypotheſis, however, as before obſerved, does not lay me under any obligation to explain how the ſun can ſuſtain the waſte of light, nor to ſhew that it will ſuſtain it for ever; and I ſhould alſo remark that, as in the analogy of generating clouds, I merely allude to their production as owing to a decompoſition of ſome of the elaſtic fluids of our atmoſphere, that analogy, which firmly reſts upon the fact, will not be leſs to my purpoſe, to whatever cauſe theſe clouds may owe their origin. It is the ſame with the lucid clouds, if I may ſo call them, of the ſun. They plainly exiſt, becauſe we ſee them; the manner of their being generated may remain an hypotheſis—and mine, till a better can be propoſed, may ſtand good; but, whether it does or not, the conſequences I am going to draw from what has been ſaid will not be affected by it.

Before I proceed I ſhall only point out, that, according to the above theory, a dark ſpot in the ſun is a place in its atmoſphere which happens to be free from luminous decompoſitions; and that faculæ are, on the contrary, more copious mixtures of ſuch fluids as decompoſe each other. The penumbra, which attends the ſpots, being generally depreſſed, more or leſs, to about half way between the ſolid body of the ſun and the upper part of thoſe regions in which luminous decompoſitions take place, muſt of courſe be fainter than other parts. No ſpot favourable for taking meaſures having lately been on the ſun, I can only judge, from former appearances, that the regions in which the luminous ſolar clouds are formed, adding thereto the elevation of the faculæ, cannot be leſs than 1843, nor much more than 2765 miles

miles in depth. It is true that in our atmoſphere the extent of the clouds is limited to a very narrow compaſs; but we ought rather to compare the ſolar ones to the luminous decompoſitions which take place in our *aurora borealis*, or luminous arches which extend much farther than the cloudy regions. The denſity of the luminous ſolar clouds, though very great, may not be exceedingly more ſo than that of our *aurora borealis*. For, if we conſider what would be the brilliancy of a ſpace two or three thouſand miles deep, filled with ſuch corruſcations as we ſee now and then in our atmoſphere, their apparent intenſity, when viewed at the diſtance of the ſun, might not be much inferior to that of the lucid ſolar fluid.

From the luminous atmoſphere of the ſun I proceed to its opaque body, which, by calculation from the power it exerts upon the planets, we know to be of great ſolidity; and from the phænomena of the dark ſpots, many of which, probably on account of their high ſituations, have been repeatedly ſeen, and otherwiſe denote inequalities in their level, we ſurmiſe that its ſurface is diverſified with mountains and vallies.

What has been ſaid enables us to come to ſome very important concluſions, by remarking, that this way of conſidering the ſun and its atmoſphere removes the great diſſimilarity we have hitherto been uſed to find between its condition and that of the reſt of the great bodies of the ſolar ſyſtem.

The ſun, viewed in this light, appears to be nothing elſe than a very eminent, large, and lucid planet, evidently the firſt, or, in ſtrictneſs of ſpeaking, the only primary one of our ſyſtem, all others being truly ſecondary to it. Its ſimilarity to the other globes of the ſolar ſyſtem with regard to its ſolidity, its atmoſphere, and its diverſified ſurface, the rotation upon its axis, and the fall of heavy bodies, leads us on to ſuppoſe that it is moſt probably alſo inhabited, like the reſt of the planets, by beings whoſe organs are adapted to the peculiar circumſtances of that vaſt globe.

Whatever fanciful poets might ſay in making the ſun the abode of bleſſed ſpirits, or angry moraliſts deviſe in pointing it out as a fit place for the puniſhment of the wicked, it does

not appear that they had any other foundation for their aſſertions than mere opinion and vague ſurmiſe; but now I think myſelf authoriſed, *upon aſtronomical principles*, to propoſe the ſun as an inhabitable world; and am perſuaded that the foregoing obſervations, with the concluſions I have drawn from them, are fully ſufficient to anſwer every objection that may be made againſt it.

It may, however, not be amiſs to remove a certain difficulty, which ariſes from the effect of the ſun's rays upon our globe. The heat which is here, at the diſtance of 95 millions of miles, produced by theſe rays, is ſo conſiderable, that it may be objected, that the ſurface of the globe of the ſun itſelf muſt be ſcorched up beyond all conception.

This may be very ſubſtantially anſwered by many proofs drawn from natural philoſophy, which ſhew that heat is produced by the ſun's rays only when they act upon a calorific medium; they are the cauſe of the production of heat, by uniting with the matter of fire which is contained in the ſubſtances that are heated; as the colliſion of flint and ſteel will inflame a magazine of gunpowder, by putting all the latent fire it contains into action. But an inſtance or two of the manner in which the ſolar rays produce their effect, will bring this home to our moſt common experience.

On the tops of mountains of a ſufficient height, at an altitude where clouds can very ſeldom reach to ſhelter them from the direct rays of the ſun, we always find regions of ice and ſnow. Now, if the ſolar rays themſelves conveyed all the heat we find on this globe, it ought to be hotteſt where their courſe is leaſt interrupted. Again, our aëronauts all confirm the coldneſs of the upper regions of the atmoſphere; and ſince, therefore, even on our earth, the heat of any ſituation depends upon the aptneſs of the medium to yield to the impreſſion of the ſolar rays, we have only to admit, that, on the ſun itſelf, the elaſtic fluids compoſing its atmoſphere, and the matter on its ſurface, are of ſuch a nature as not to be capable of any exceſſive affection from its own rays: and, indeed, this ſeems to be proved by the copious emiſſion of them; for if the elaſtic fluids of the atmoſphere, or the matter contained on the ſurface of the ſun, were of ſuch a nature

as to admit of an eaſy, chemical combination with its rays, their emiſſion would be much impeded.

Another well known fact is, that the ſolar focus of the largeſt lens, thrown into the air, will occaſion no ſenſible heat in the place where it has been kept for a conſiderable time, although its power of exciting combuſtion, when proper bodies are expoſed, ſhould be ſufficient to fuſe the moſt refractory ſubſtances *.

It will not be neceſſary to mention other objections, as I can think of none that may be made but what a proper conſideration of the foregoing obſervations will eaſily remove; ſuch as may be urged from the diſſimilarity between the luminous atmoſphere of the ſun and that of our globe will be touched upon hereafter, when I conſider the objections that may be aſſigned againſt the moon's being an inhabitable ſatellite.

I ſhall now endeavour, by analogical reaſonings, to ſupport the ideas I have ſuggeſted concerning the conſtruction and purpoſes of the ſun; in order to which it will be neceſſary to begin with ſuch arguments as the nature of the caſe will admit, to ſhew that our moon is probably inhabited. This ſatellite is of all the heavenly bodies the neareſt, and therefore moſt within the reach of our teleſcopes: accordingly we find, by repeated inſpection, that we can with perfect confidence give the following account of it:—

It is a ſecondary planet, of a conſiderable ſize; the ſurface of which is diverſified, like that of the earth, by mountains and vallies. Its ſituation with reſpect to the ſun is much like that of the earth, and, by a rotation on its axis, it enjoys an agreeable variety of ſeaſons, and of day and night. To the moon our globe will appear to be a very capital ſatellite, undergoing the ſame regular changes of illuminations as the moon does to the earth. The ſun, the planets, and the ſtarry conſtellations of the heavens, will riſe and ſet there as they do here; and heavy bodies will fall on the moon as they do

* The ſubject of light and heat has been very ably diſcuſſed by Mr. De Luc, in his excellent work, *Idées ſur la Météorologie*, Tome I. part 2. chap. 2. ſection 2. *De la Nature du Feu*; and Tome II. part 3. chap. 6. ſection 2. *Des Rapports de la Lumière avec la Chaleure dans l'Atmoſphère.*

on the earth. There ſeems only to be wanting, in order to complete the analogy, that it ſhould be inhabited like the earth.

To this it may be objected, that we perceive no large ſeas in the moon: that its atmoſphere (the exiſtence of which has even been doubted by many) is extremely rare, and unfit for the purpoſes of animal life: that its climates, its ſeaſons, and the length of its days, totally differ from ours: that without denſe clouds (which the moon has not), there can be no rain; perhaps no rivers, no lakes. In ſhort, that, notwithſtanding the ſimilarity which has been pointed out, there ſeems to be a decided difference in the two planets we have compared.

My anſwer to this will be, that that very difference which is now objected will rather ſtrengthen the force of my argument than leſſen its value: we find, even upon our globe, that there is the moſt ſtriking difference in the ſituation of the creatures that live upon it. While man walks upon the ground, the birds fly in the air, and fiſhes ſwim in water; we can certainly not object to the conveniences afforded by the moon, if thoſe that are to inhabit its regions are fitted to their conditions as well as we on this globe are to ours. An abſolute, or total ſameneſs, ſeems rather to denote imperfections, ſuch as Nature never expoſes to our view; and, on this account, I believe the analogies that have been mentioned fully ſufficient to eſtabliſh the high probability of the moon's being inhabited like the earth.

To proceed, we will now ſuppoſe an inhabitant of the moon, who has not properly conſidered ſuch analogical reaſonings as might induce him to ſurmiſe that our earth is inhabited, were to give it as his opinion that the uſe of that great body, which he ſees in his neighbourhood, is to carry about his little globe, that it may be properly expoſed to the light of the ſun, ſo as to enjoy an agreeable and uſeful variety of illumination, as well as to give it light by reflection from the ſun when direct day-light cannot be had. Suppoſe alſo that the inhabitants of the ſatellites of Jupiter, Saturn, and the Georgian planet, were to look upon the primary ones, to which they belong, as mere attractive centres, to keep together their orbits, to direct their revolution round the ſun, and

to

to ſupply them with reflected light in the abſence of direct illumination. Ought we not to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the earth, and thoſe other planets that have ſatellites about them, perform all the offices that have been named for the inhabitants of theſe little globes; but to us, who live upon one of theſe planets, their reaſonings cannot but appear very defective, when we ſee what a magnificent dwelling-place the earth affords to numberleſs intelligent beings.

Theſe conſiderations ought to make the inhabitants of the planets wiſer than we have ſuppoſed thoſe of their ſatellites to be. We ſurely ought not, like them, to ſay "the ſun "(that immenſe globe, whoſe body would much more than "fill the whole orbit of the moon) is merely an attractive "centre to us." From experience we can affirm, that the performance of the moſt ſalutary offices to inferior planets is not inconſiſtent with the dignity of ſuperior purpoſes; and, in conſequence of ſuch analogical reaſonings, aſſiſted by teleſcopic views, which plainly favour the ſame opinion, we need not heſitate to admit that the ſun is richly ſtored with inhabitants.

This way of conſidering the ſun is of the utmoſt importance in its conſequences. That ſtars are ſuns can hardly admit of a doubt. Their immenſe diſtance would perfectly exclude them from our view, if the light they ſend us were not of the ſolar kind. Beſides, the analogy may be traced much farther. The ſun turns on its axis: ſo does the ſtar Algol: ſo do the ſtars called β Lyræ, δ Cephei, η Antinoi, o Ceti, and many more; moſt probably all. From what other cauſe can we ſo probably account for their periodical changes? Again, our ſun has ſpots on its ſurface: ſo has the ſtar Algol, and ſo have the ſtars already named, and probably every ſtar in the heavens. On our ſun theſe ſpots are changeable: ſo they are on the ſtar o Ceti, as evidently appears from the irregularity of its changeable luſtre, which is often broken in upon by accidental changes while the general period continues unaltered. The ſame little deviations have been obſerved in other periodical ſtars, and ought to be aſcribed

aſcribed to the ſame cauſe. But if ſtars are ſuns, and ſuns are inhabitable, we ſee at once what an extenſive field for animation opens itſelf to our view.

It is true that analogy may induce us to conclude, that, ſince ſtars appear to be ſuns, and ſuns, according to the common opinion, are bodies that ſerve to enlighten, warm, and ſuſtain a ſyſtem of planets, we may have an idea of numberleſs globes that ſerve for the habitation of living creatures. But if theſe ſuns themſelves are primary planets, we may ſee ſome thouſands of them with our own eyes, and millions by the help of teleſcopes, when at the ſame time the ſame analogical reaſoning ſtill remains in full force with regard to the planets which theſe ſuns may ſupport.

In this place I may, however, take notice, that, from other conſiderations, the idea of ſuns or ſtars being *merely* the ſupporters of ſyſtems of planets, is not abſolutely to be admitted as a general one. Among the great number of very compreſſed cluſters of ſtars I have given in my catalogues, there are ſome which open a different view of the heavens to us. The ſtars in them are ſo very cloſe together, that, notwithſtanding the great diſtance at which we may ſuppoſe the cluſter itſelf to be, it will hardly be poſſible to aſſign any ſufficient mutual diſtance to the ſtars compoſing the cluſter, to leave room for crowding in thoſe planets, for whoſe ſupport theſe ſtars have been, or might be, ſuppoſed to exiſt. It ſhould ſeem, therefore, highly probable that they exiſt for themſelves; and are, in fact, only very capital, *lucid*, primary planets, connected together in one great ſyſtem of mutual ſupport.

As in this argument I do not proceed upon conjectures, but have actual obſervations in view, I ſhall mention an inſtance in the cluſters No. 26, 28, and 35, VI. claſs, of my catalogue of nebulæ, and cluſters of ſtars. (See Phil. Tranſ. Vol. LXXIX. Part II. p. 251.) The ſtars in them are ſo crowded that I cannot conjecture them to be at a greater apparent diſtance from each other than five ſeconds, even after a proper allowance for ſuch ſtars, as, on a ſuppoſition of a globular form of the cluſter, will interfere with one another, has been made. Now, if we would leave as much room between

tween each of theſe ſtars as there is between the Sun and Sirius, we muſt place theſe cluſters 42104 times as far from us as that ſtar is from the ſun. But, in order to bring down the luſtre of Sirius to that of an equal ſtar placed at ſuch a diſtance, I ought to reduce the aperture of my 20-feet teleſcope to leſs than the two-and-twenty hundredth part of an inch; when certainly I could no longer expect to ſee any ſtar at all.

The ſame remark may be made with regard to the number of very cloſe double ſtars, whoſe apparent diameters being alike, and not very ſmall, do not indicate any very great mutual diſtance: from which, however, muſt be deducted all thoſe where the different diſtances may be compenſated by the real difference in their reſpective magnitudes.

To what has been ſaid may be added, that, in ſome parts of the milky way, where yet the ſtars are not very ſmall, they are ſo crowded, that in the year 1792, Aug. 22, I found by the gages that, in 41 minutes of time, no leſs than 258 thouſand of them had paſſed through the field of view of my teleſcope *.

It ſeems, therefore, upon the whole, not improbable that, in many caſes, ſtars are united in ſuch cloſe ſyſtems as not to leave much room for the orbits of planets or comets; and that conſequently, upon this account alſo, many ſtars, unleſs

* The ſtar-gages ran thus:

From	19^h	$35'$ to	19^h	$51'$ 600 ſtars in the field
	19	51 —	19	57 440
	19	57 —	20	12 360
	20	12 —	20	16 260

The breadth of the ſweep was 2° $35'$, the diameter of the field $15'$, and the mean polar diſtance 73° $54'$. Then let

F, be the diameter of the field of view,
S, the number of ſtars in each field,
B, the breadth of the ſweep, plus F,
T, the length of the ſweep, expreſſed in minutes of ſpace,
φ, the ſine of the mean polar diſtance,
C, the conſtant fraction ,7854,

and the ſtars in theſe four ſucceſſive ſhort ſweeps will be found by the expreſſion $\frac{BTS\varphi}{FFC}$ equal to 153095. 36601. 74766. 14419. or in all 258981.

We

we would make them mere uſeleſs brilliant points, may themſelves be lucid planets, perhaps unattended by ſatellites.

POSTSCRIPT.

The following obſervations, which were made with an improved apparatus, and under the moſt favourable circumſtances, ſhould be added to thoſe which have been given. They are deciſive with regard to one of the conditions of the lucid matter of the ſun.

Nov. 26, 1794. Eight ſpots in the ſun, and ſeveral ſubdiviſions of them, are all equally depreſſed.

The ſun is mottled every where.

The mottled appearance of the ſun is owing to an inequality in the level of the ſurface.

The ſun is equally mottled at its poles and at its equator; but the mottled appearances may be ſeen better about the middle of the diſc than towards the circumference, on account of the ſun's ſpherical form.

The unevenneſs ariſing from the elevation and depreſſion of the mottled appearance on the ſurface of the ſun, ſeems in many places to amount to as much, or to nearly as much, as the depreſſion of the penumbræ of the ſpots below the upper part of the ſhining ſubſtance, without including faculæ, which are protuberant.

The lucid ſubſtance of the ſun is neither a liquid nor an elaſtic fluid; as is evident, from its not inſtantly filling up the cavities of the ſpots, and of the unevenneſs of the mottled parts. It exiſts, therefore, in the manner of lucid clouds ſwimming in the tranſparent atmoſphere of the ſun; or rather, of luminous decompoſitions taking place within that atmoſphere.

IV. *Extract of a Memoir, and Experiments on the Nutrition of Plants. By* M. Rafn, *Aſſeſſor in the Office of Commerce at Copenhagen* *.

Hassenfratz conſiders carbon as the ſubſtance which nouriſhes vegetables †. Ingenhous, in his work on the nutrition of plants, publiſhed in 1797, endeavours to prove, that if carbon has any influence in this reſpect, it can be only in the ſtate of carbonic acid, as that acid is abſorbed and decompoſed ‡ by vegetables; while the ligneous carbon, furniſhed by Nature, produces no effect on the expanſion of plants. Mr. A. Young has endeavoured to demonſtrate the ſame thing by experiments. M. Rafn, deſirous of diſcovering the truth amidſt theſe contradictory opinions, made, for three years, a ſeries of experiments, from which he concludes, by the expanſion, ſize, and colour of the plants employed, that carbon, either vegetable or animal, has a decided influence in the nouriſhment of vegetables. What is new, and particularly worthy of remark in theſe reſearches, is, that, according to M. Rafn, the carbonic acid produces exactly the ſame effect as charcoal of wood. The following are the experiments which conducted the author to this reſult:—

Having half filled a large box with brick-kiln rubbiſh, or pounded tiles, which he covered with a layer of vegetable earth, he placed over the latter a ſtratum of carbonate of lime (pounded limeſtone) and alum, and then two or three of vegetable mould, in which he ſowed barley. He preſumed that the ſulphuric acid of the alum, abandoning the argil to join the lime, with which this acid has greater affi-

* Tranſlated from the Daniſh, with notes by C. Vauquelin and Brogniart.

† This opinion of C. Haſſenfratz appears to be very probable; but, as he ſays, the carbon muſt be held in ſolution by hydrogen gas, by water, by that ſaponaceous extract which ſeparates from vegetables when in a ſtate of putrefaction, or by any other liquid.

‡ This decompoſition is poſſible, but it has not yet been proved by any direct experiment.

nity, the carbonic acid gas would be diſengaged, which would furniſh the means of knowing its influence on the vegetation. Another box was filled merely with mould, a third merely with charcoal, and a fourth with animal carbon. Theſe were to be employed in comparative experiments, and barley was ſown in them all.

Though the plants which germinated in the firſt box were ſown in a ſtratum of mould about two or three inches in thickneſs, they had no reſemblance, either in ſtrength or colour, to thoſe ſown in the ſecond box filled with mould alone; but they had, on the other hand, ſuch a perfect reſemblance to thoſe of the third box filled with charcoal, that it would have been difficult to diſtinguiſh the difference. This reſemblance continued ſeveral weeks, after which they ſeemed to have not quite the ſame vigour as thoſe which grew in the charcoal, for which it is not difficult to aſſign a reaſon. The author convinced himſelf that a decompoſition had really taken place, becauſe, on examining the firſt box in autumn, he found that ſulphat of lime had been formed. Theſe experiments ſeem proper to conduct to a knowledge of the manner in which plants attract the carbonic principle, which all the reſearches of the author demonſtrate to be neceſſary for vigorous vegetation. He propoſes to repeat them on a larger ſcale, and to vary them as much as poſſible*. He repeated, ſeveral times, thoſe of M. Humboldt on germination, accelerated by the oxygenated muriatic acid, and always with ſucceſs, though with this difference, that this acid did not favour vegetation ſo much as that philoſopher aſſerted.

M. Raſn ſowed barley in a mixture of mould, ſand, and manganeſe, in order to ſee whether the oxygen gas would

* Theſe experiments would be more concluſive had not the author added mould in the boxes into which he put the rubbiſh. It is well known that mould contains a great quantity of carbon, exactly in the ſtate which renders it fit for the nutrition of vegetables.

M. A. Young, on the other hand, aſſerts, that plants grow exceedingly ill in charcoal; and this obſervation agrees more with the others, and with the reaſoning, which induces us to believe that carbon muſt be diſſolved to enter into combination with the other principles of vegetables. As plants grow exceedingly well in pure water till a certain period, it would appear that they ought to grow equally well in watered charcoal.

not

not be disengaged in such a manner as to produce some effect on plants. At first he obtained no effect; but having watered this box with diluted sulphuric acid, he remarked that the barley visibly grew faster in this box than in those not watered in the same manner *.

Of all the mixtures which he tried for sowing, none appeared to him better than that of equal parts of charcoal, mould, and sand, moistened with water filled with infusion animals, which may be easily obtained by steeping flax in the water destined for that purpose. He observes, on this occasion, that, of all the substances he tried, flax is that which furnishes the most of these animalculæ. An incredible multitude of them are found in the water in which women dip their fingers when they are employed in spinning. The water put into a vessel for that use in the morning, is found filled with them in the evening. The author ascribes to these small animals a much greater influence on vegetation than has hitherto been believed.

Hassenfratz relates, that he could not make plants vegetate well in simple earths. The author asserts, that he had great success when he reared them in pure silex, quartzy sand thrice washed, fine sand from the sea-shore, &c. But these plants continued stunted and pale, and their roots were twice as long as the whole of the part above the earth. In charcoal, on the other hand, the parts were large and vigorous; they were of an exceedingly dark colour, and their roots were not a sixth part of the length of the plant itself †.

Coal-ashes, on which the German and English farmers bestow such praise, destroy the plants if the soil contains an eighth part of that admixture. The leaves become faded, as

* The sulphuric acid cold does not disengage the oxygen of the oxyd of manganese: besides, according to the experiments of Ingenhous, this acid alone, in small quantity, seems to have the property of rendering vegetation more active.

† The first results are perfectly similar to those obtained by C. Hassenfratz. In regard to the second, they depend on the purity of the charcoal employed, which may contain wood undecomposed, and consequently disposed to putrify, and to yield a liquid which may hold the carbon in solution.

if ſcorched, at the end of from fifteen to twenty days, and the plants themſelves die at the end of four or five weeks.

No ſeed germinates in oil. A ſingle grain of common ſalt in two hundred grains of water is ſufficient to retard the vegetation of plants, and may even kill them if they are watered with that ſaline liquor *.

Shavings of horn, next to infuſion animals, are the moſt favourable to vegetation: charcoal holds the third rank.

V. *On the Aſſaying of Iron Ores and Iron-Stones by Fuſion.* *By Mr.* DAVID MUSHET, *of the Clyde Iron Works* †.

IT will eaſily be conceived, from the mode of operation which I have adopted, that, in order to procure accurate reſults, the proportion of flux muſt be varied according to the mixtures in the iron-ſtones or ores; and that no univerſal ſolvent can be uſed as capable of aſſaying *all* ores.

As the gradation of mixtures in the ores is almoſt imperceptible, there are, in fact, no fixed limits by which Nature has diſtinguiſhed the various claſſes: we find all the varieties diminiſhing their predominant earth, and aſſuming, in equal proportions, thoſe of each other, thus conſtituting the claſs of equaliſed mixtures; yet, here, the variety of combination ceaſes not, the predominating earth gradually becomes the minor part of the mixture, and that which only held a ſecond rank, as to quantity, is now the chief component earth; the permutation goes round, till the earth, which exiſted in the moſt ſparing quantity, now predominates to exceſs.

* C. Sylveſtore obtained a reſult abſolutely ſimilar, by employing marine ſalt as manure.

† The preſent is a part of the communication from Mr. Muſhet which appeared in our Number for July laſt. (Vol. IV. p. 178,) but by an overſight of the Editor was omitted in its place. It contains the table of proportions alluded to in our Number for September laſt (Vol. IV. p. 380.) requiſite for the obtaining from all the various iron-ſtones an accurate aſſay, and ſhould have immediately followed Mr. Muſhet's article given in our July number.

In

In ſuch an infinity of variation it is difficult to arrange the combinations of which theſe ſubſtances are capable. To derive the name of a claſs, or genus, from the predominancy of an earth, ſeems moſt eligible; and to conſider thoſe as varieties of the ſame claſs, which are altered by the proportion of the ſecond and third mixtures. Again, each of theſe varieties are ſuſceptible of a multiplicity of modifications before an earth is ſo far diminiſhed as to give an aſcendency to another, or before the third rank of proportion has aſſumed that of the ſecond or firſt. The ſimple combination of the earths, and their degrees of predominancy, may be thus arranged:—

	1ſt Variety,		2d Variety.	
Argillaceous iron-ſtone	Iron	- -	Iron	As theſe become varied, they form the claſs of equalized mixture.
	Clay	- -	Clay	
	Lime	- -	Silex	
	Silex	- -	Lime	
Calcareous iron-ſtone	Iron	- -	Iron	
	Lime	- -	Lime	
	Clay	- -	Silex	
	Silex	- -	Clay	
Siliceous iron-ſtone	Iron	- -	Iron	
	Silex	- -	Silex	
	Lime	- -	Clay	
	Clay	- -	Lime	

To aſſay any of theſe varieties, a flux peculiar to the nature of the mixture is neceſſary; ſo that the changes of proportion in the ſolvent ought to extend to ſeven, including the claſs of equaliſed mixtures, in order that the preciſe ſame quality of crude iron may be produced from all the varieties of iron-ſtone. The modification of each variety will be found to be ſufficiently accurate, if aſſayed by the flux peculiar to itſelf. The arrangement of the three claſſes of ores into two varieties, each forming a diſtinct ſtage of combination, indicated by the predominancy of the firſt and ſecond earth, are, with the neutral claſs, ſufficiently minute for any purpoſe in the aſſay-furnace, and are ſufficient to form an

accurate and extenſive knowledge of the analogy of theſe reſults with thoſe in the blaſt-furnace.

Table *of* Proportions *of* Fluxes.

Let the earthy part of an argillaceous ore be compoſed of clay 9, lime 6, ſand 3 = 18.

To aſſay 4 troy ounces of this ore			- or	1920 grains,
add	4	———	bottle glaſs	— 1920
	3	———	chalk	— 1440
	$0\frac{1}{2}$	———	charcoal	— 240
	$11\frac{1}{2}$			5520

Let the ſecond variety of argillaceous ores contain, clay 10, ſilex 7, lime 3 = 20.

In this caſe, 4 ounces troy of ore			- or	1920 grains,
would require	4	———	bottle glaſs	— 1920
	4	———	chalk	— 1920
	$0\frac{3}{4}$	———	charcoal	— 360
	$12\frac{3}{4}$			6120

Let the firſt variety of the calcareous genus of iron-ſtone be ſuppoſed to contain, of earthy mixtures, lime 14, clay 6, ſilex 4 = 24.

When this iron-ſtone is to be aſſayed,

to	4 ounces troy		- - or	1920 grains,
add	5	———	bottle glaſs -	— 2400
	$1\frac{1}{2}$	———	chalk —	— 720
	$0\frac{1}{4}$	———	charcoal —	— 360
	$11\frac{1}{4}$			5400

Again, let the ſecond variety of the calcareous genus be ſuppoſed to contain, lime 10, ſand 6, clay 4 = 20.

I would add to	4 troy ounces		- or	1920 grains,
	4	———	bottle glaſs	— 1920
	2	———	chalk	— 960
	$0\frac{1}{2}$	———	charcoal	— 240
	$10\frac{1}{2}$			5040

Let

Let the firſt variety of filiceous ores be ſuppoſed to contain, ſilex 12, clay 8, lime 5 = 25.

For an aſſay of	4 troy ozs. of ore	-	or	1920 grains,
add	4 ———	chalk	—	1920
	3 ———	bottle glaſs	—	1440
	$0\frac{3}{4}$ ———	charcoal	—	360
	$11\frac{3}{4}$			5640

And, laſtly, let the ſecond variety of this genus of iron-ſtone be ſuppoſed to contain, ſilex 10, lime 7, clay 5 = 22.

To	4 troy ounces of the ore	-	or	1920 grains,
add	$3\frac{1}{2}$ ———	chalk	—	1680
	3 ———	bottle glaſs	—	1440
	$0\frac{3}{4}$ ———	charcoal	—	360
	$11\frac{1}{4}$			5400

Claſs of equaliſed mixtures, compoſed of, clay 7, lime 7, ſilex 7 21.

To aſſay of this ore	4 troy ounces	-	or	1920 grains,
add	$3\frac{1}{2}$ ———	bottle glaſs	—	1680
	$2\frac{1}{2}$ ———	chalk	—	1200
	$0\frac{1}{2}$ ———	charcoal	—	240
	$10\frac{1}{2}$			5040

VI. *On the real Origin of that Reſin known under the Name of Sandarac, and that of Gum Arabic. By* M. SCHOUSBOE *.

GUM Sandarac is an article of trade brought from the ſouthern provinces of the kingdom of Morocco. About ſix or ſeven hundred quintals of it are exported every year from Santa Cruz, Mogador, and Saffy. In the language of the country it is called *El graſſa*. The tree which produces it is a *Thuia*, found alſo by M. Vahl in the kingdom of

* From a Daniſh Journal, entitled, *The Phyſical, Medical, and Economical Library*, Part III. 1799.

Tunis.

Tunis *. It was made known feveral years ago by Dr. Shaw, who named it, *Cypreſſus fructu quadrivalvi, Equiſeti inſtar articulatis*; but neither of theſe learned men was acquainted with the economical uſe of this tree; probably becauſe, being not common in the northern part of Barbary, the inhabitants find little advantage in collecting the reſin which exudes from it. This reſin hitherto has been aſcribed to the *Juniperus communis*, *Juniperus Lycia*, or the Cedar of Lebanon, without reflecting that the *Juniperus communis* does not grow in Africa, and Sandarac ſeems to belong excluſively to that part of the world. M. Schouſboe, who ſaw the ſpecies of *Thuia* in queſtion, ſays that it does not riſe to more than the height of twenty or thirty feet at moſt, and that the diameter of its trunk does not exceed ten or twelve inches. It diſtinguiſhes itſelf, on the firſt view, from the two other ſpecies of the ſame genus, cultivated in gardens, by having a very diſtinct trunk, and the figure of a real tree; whereas in the latter the branches riſe from the root, which gives them the appearance rather of buſhes. Its branches alſo are more articulated and brittle. Its flowers, which are not very apparent, ſhew themſelves in April; and the fruit, which are of a ſpherical form, ripen in September. When a branch of this tree is held to the light, it appears to be interſperſed with a multitude of tranſparent veſicles which contain the reſin. When theſe veſicles burſt in the ſummer months, a reſinous juice exudes from the trunk and branches, as is the caſe in other coniferous trees. This reſin is the Sandarac, which is collected by the inhabitants of the country, and carried to the ports, from which it is tranſported to Europe. It is employed in making ſome kinds of ſealing-wax, and in different ſorts of varniſh. In 1793 a hundred weight of it coſt in Morocco from 13 to 13½ piaſtres, which make about 3*l.* 5*s.* to 3*l.* 7*s.* and 6*d.* ſterling. The duty on exportation was about 7*s.* 6*d.* ſterling *per* quintal.

Sandarac, to be good, muſt be of a bright-yellow colour, pure and tranſparent. It is an article very difficult to be

* A complete deſcription of it, with a good figure, may be found in his work entitled, *Symbol. Botan.* Part II. p. 96. Plate XLVIII. under the name *Thuia articulata*.

adulterated.

adulterated. Care, however, muſt be taken that the Moors do not mix with it too much ſand. It is probable that a tree of the ſame kind produces the gum ſandarac of Senegal, which is exported in pretty conſiderable quantities.

Another article of commerce in which the kingdom of Morocco participates with Senegal, is that gum called Gum Arabic, known by the name of *Al leilk*. The tree which produces it grows only in the ſouthern provinces of that ſtate. The quantity of this article exported to the different parts of Europe from the ports of Morocco may amount to about eight or nine quintals. M. Schouſboe ſays, that this tree is the *Mimoſa nilotica*, named in the country *Al thlah*; but this is no reaſon why the ſame kind of gum ſhould not be collected in the more ſouthern countries of Africa from the *Mimoſa Senegal*, and even from other trees of the ſame ſpecies, as we are told by various authors. In Barbary the people make a difference between the gum of Senegal and that of the country. The former is preferred on account of its purity, whiteneſs, and tranſparency, which in general are the properties ſought for in this article.

The gum which I collected myſelf in the province of Mogador, ſays M. Schouſboe, exudes from the trunk and branches of the tree, in the ſame manner as that of our fruit-trees. It is in round lumps, of the ſize of a hazle-nut, or rather that of a walnut. Theſe lumps, indeed, by becoming united to each other, form maſſes ſometimes of the ſize of the fiſt, or even of the head; but this only happens in conſequence of the adheſion which takes place between the pieces of gum, when ſtill freſh, after they have been detached from the tree, and chiefly at that part which was attached to the bark, where the reſinous juice has not had time to harden. If earth, ſmall ſtones, and other foreign bodies, are ſometimes found in theſe maſſes, it ariſes from fraud; and M. Schouſboe ſuſpects that this circumſtance has given riſe to the opinion of the gum being found at the bottom of the tree, and that it exudes from the roots, which, as he thinks, is void of foundation. Were this the caſe, it appears to him that, beſides the earth and ſand with which the gum is accidentally dirtied, balls of theſe matters ought to be found

in the inſide, and even ſo mixed with the mucilaginous ſubſtance that it would be impoſſible ever to purify it completely; while, on the other hand, the gum which comes from Senegal is ſtill purer than that of Barbary.

M. Schouſboe, however, obſerves, that the ſandarac and gum exported from the port of Saffy have a brown or reddiſh colour; but he aſcribes this colour to the quantity of the red oxyd of iron mixed with the ſoil of the province of Abda, where this port is ſituated. This oxyd communicates its colour even to the whiteſt wool; and the inhabitants of that province may be diſtinguiſhed by the reddiſh tint of their clothes, which cannot be entirely deſtroyed by any proceſs. In the months of July and Auguſt, when heavy dews fall, the gum loſes a great deal of its tranſparency, as well as of its other good qualities. A hundred weight of this ſubſtance coſt at Mogador, in 1793, about 2 *l.* ſterling, without including about 4*s.* cuſtom-houſe duty. The gum does not appear to be employed by the inhabitants of Morocco for any purpoſe whatever: the whole of what they collect is ſold to the different commercial nations of Europe.

VII. *On the Peſtilential Diſeaſes which, at different times, appeared in the Athenian, Carthagian and Roman Armies, in the Neighbourhood of Syracuſe. By the late* E. H. Smith, *Phyſician* *.

Section I.

SYRACUSE, the moſt beautiful of all the cities built by the Greeks, was founded by Archias, a Corinthian, of the race of Hercules *. He firſt expelled the natives from Ortygia, where he commenced the city, which was afterwards extended to the neighbouring continent. This place, ſo celebrated in ancient hiſtory, the birth-place of Archimedes, and theatre of many memorable tranſactions, now reduced to a miſerable town, of inferior conſequence even in Sicily, is ſituated in north latitude 37° 5′, a little above Cape Paſſara

* From the *American Medical Repoſitory*, Vol. II. No. 4.

† Cicero in Ver. Act. II. Lib. iv § 117. Thucydides, B. vi.

(the

(the ancient Pachynum), the ſouth-weſtern extremity of the iſland of Sicily.

The climate of Sicily, in general, is repreſented as favourable to health *; and, notwithſtanding its inſular ſituation, more analogous, in the qualities of temperature and humidity, to the hilly, than to the Atlantic diviſions of South-Carolina and Georgia. The winter is remarkably mild: with the exception of a few days, it equals the fineſt ſpring weather in the North of Europe †, and the Eaſtern States of America; and the ſhade is found pleaſant, in the middle of the day, even in the month of January ‡. At Syracuſe, in particular, the ſeaſon is ſo little affected by the ſeverities of cold and tempeſt, that, during its whole courſe, and in its moſt boiſterous ſtate, the ſky is never totally obſcured for a ſingle day §. Yet in this climate did Verres, the voluptuous prætor of Sicily, that monſter of atrocity and laſciviouſneſs, ſo entirely ſeclude himſelf from all impact and influence of the atmoſphere, as only to learn the approach of ſpring by obſerving the dew gliſten on the verdure which ſurrounded his palace ‖. As the ſeaſon advances, the heat rapidly increaſes, till, in the ſummer, it is no longer to be endured by ſtrangers, and exertion, as well as enjoyment, is temporarily ſuſpended. In autumn, the frequent rains, which are common throughout this ſeaſon, and the heats of the middle of the day, contraſted with the extreme chilline's of the evenings, render it leſs pleaſant and ſalubrious than any other part of the year ¶. It is to the exceſſive ſultrineſs of the ſummers that a late ſenſible writer ** aſcribes the imperfection of many,

* Benige Berichten, &c. *i. e.* Some account of the Pruſſian, Auſtrian, and Sicilian monarchies, &c. See Monthly Review Enlarged, Vol. XIV. p. 491.

† Swinburne's Travels in the Two Sicilies, Vol. II. § 49. Dubl. edit.

‡ Keyſler's Travels, Vol. III p. 33.—Tranſlation.

§ Cicero in Ver. Act. II. Lib. v. § 26.

‖ Cicero in Ver. Act. II. Lib. v. § 12.

¶ Travels in Sicily and Malta, by M. De Non, p. 307, 333, and 383.—Tranſlation.

** Benige Berichten, &c.—"The ſoil is exceedingly fertile; but, from the great heat of the climate, many of our moſt nouriſhing and refreſhing

many, and the total want of others, of the moſt nutritious and refreſhing vegetables of the northern climates, in deſpite of that fertility of ſoil which, from time immemorial, has conferred on Sicily the appellation of the granary of Europe.

But, notwithſtanding the general pleaſantneſs and healthfulneſs of this iſland, the concurrent teſtimony of ancient and modern writers evinces the noxious condition of particular places. "The leaſt ſtagnant water is ſufficient, in the heats of ſummer, to poiſon the atmoſphere: its effects on the countenances of the poor people, who live in its vicinity, are very evident; and a ſtranger, who travels through the iſland in this ſeaſon, ought to avoid ever paſſing a night near them *. As ſoon as the ſun enters the Lion, this country becomes the houſe of death: fevers, of the moſt malignant kind, ſeize upon the imprudent or unfortunate wretch that ſpends a night near them (ponds and marſhes); and few eſcape with life, when attacked by ſo virulent a diſorder †." Inſtructed, probably, by experience of the calamities conſequent on a near reſidence to marſhes and ſtagnant waters, the inhabitants in various parts of Sicily have built their towns on adjoining eminences ‡. To avoid the dangers which befet the ſtranger in journeying through this country in the ſultry and autumnal ſeaſons, was probably one of the motives of the Roman prætors for performing the tour of the iſland in the time of harveſt §. By a neglect of ſimilar prudence, the celebrated M. De Non became affected with a violent fever and ague, after an incautious expoſure to the heat of the ſun and the chill of the evening, in an unwholeſome part of the country, and in the month of September ‖.

From this brief account of the climate of Sicily, it will be evident, that, how favourable ſoever it may be eſteemed, in general, to the health and longevity of the natives, and how

vegetables will not thrive in it. Currants, raſpberries, and gooſeberries, are unknown to the natives; and foreigners, who have attempted to cultivate them, have never been able to ſucceed."

* Eenige Berichten, &c.

† Swinburne's Travels, Vol. II. § 49. Dublin edit.

‡ Swinburne's Travels, Vol. II. p. 300. Dublin edit.

§ Cicero in Ver. Act II. Lib. v. § 80.

‖ De Non's Travels, p. 383.—Tranſlation.

pleaſant foever many parts of it deſerve to be conſidered as a winter and even vernal reſidence for ſtrangers; yet, in other ſeaſons, and particularly for viſitors from colder countries, it muſt be in moſt parts unſalutary, in others certainly fatal, and, in ſome, not to be long continued in, with impunity, by the inhabitants themſelves. And, as this remark refers wholly to ordinary years and circumſtances, it will afford ſome ground for eſtimating the effects of one of the moſt unhealthy ſituations in the country, in the ſickly ſeaſon of the year, with a conſtitution of the atmoſphere favourable to epidemic diſeaſes, on an army of foreigners, tumultuous and ill-accommodated, worn down with fatigue, or ſinking in deſpondency.—With this reflection impreſſed on the mind, we may now proceed to a deſcription of the city of Syracuſe, and the country immediately adjacent, as they exiſted in ancient times, occaſionally ſupplying illuſtrations from the accounts of modern travellers.

The once beautiful city of Syracuſe conſiſted of five principal diviſions, traces of all which are ſtill diſcernible. 1. The iſle Ortygia, Naſos, or Naſon: 2. Achradina: 3. Tyche, or Tycha: 4. Neapolis, or the new city: and, 5. Epipolæ *.

Ortygia, encloſed by the two ports—the great port on the weſt and the little port on the eaſt, and connected with the continent by a bridge—is of an oblong form, and about two miles in circumference †. Here the original ſettlement commenced, and this was always conſidered as the wealthieſt and moſt deſirable part of Syracuſe. It was the ancient reſidence of the kings; and, in the time of Cicero, ſtill contained the houſe of Hiero. It was ornamented with various public buildings, particularly the temples of Diana and Minerva; and, in part, watered by the poetic fountain of Arethuſa. As the population increaſed, the ſound, which divided Ortygia from the Continent, was filled up, and the iſle converted into a peninſula ‡. Carlos III. of Spain removed the earth by which they were united; and Ortygia is again an iſland, and connected with the Continent, as formerly, by a bridge §.

* Cicero in Ver. Act. II. Lib. iv. § 118. De Non, p. 304. et ſequent. Swinburne, Vol. II. p. 309.

† De Non. Swinburne.

‡ De Non. Swinburne.

§ Brydone's Tour, Letter xii.

The

The ſituation of Ortygia is important, for it commands both the ports; and though ſuppoſed itſelf to be commanded by Achradina, yet, while that quarter of the city remained in the poſſeſſion of the Syracuſans, together with the iſle, and the oppoſite promontory of Plemmyrium, the town was nearly inacceſſible to a naval force *. At preſent Ortygia is the only city. To this ſtate it was reduced by the Muſſulmen. It is ſtrongly defended towards the land, weakly towards the ſea-ſide. Its quay is ſmall; its ſtreets narrow, winding, and wretchedly built; and its population does not exceed 18,000 perſons †. The fountain of Arethuſa, after repeated changes of ſituation ‡, is ſtill diſcoverable in the weſt part of the iſle; but its beauty and its honours have fled with the mythology to which it is indebted for its fame.

Achradina. The quarter of Achradina, at the period to which this eſſay has particular reference, was the moſt ſpacious, well built, and ſtrongly fortified part of the city. It extends over two conſiderable levels, divided by a natural wall of calcareous rocks; the one as elevated as Tyche; the other, and more conſiderable, on a plane with Ortygia, and thence conferring on this quarter the character of the loweſt diviſion of Syracuſe. The eaſtern part was the moſt commodious, and not leſs extenſive than the modern Paris. The whole was adorned with a large forum, a beautiful portico, prytaneum, curia, and the temple of Jupiter Olympus; bordered on three ſides by the ſea, by the great port on the weſt, the leſſer on the ſouth, and the port of Trogilus on the eaſt; and, on all ſides, nearly impregnable. The rocks of this quarter of Syracuſe, which are formed by marine depoſitions, poſſeſs the ſingular property of diſſipating or abſorbing the moiſture of dead bodies ſo ſpeedily, that they are preſerved in vaults excavated for the purpoſe, in their proper form and habiliments. Achradina is remarkably fertile, and naturally adapted for bringing to perfection every tropical production §.

* De Non. Swinburne.

† Swinburne, Vol. II. p. 311, 312. De Non, p. 304.

‡ De Non, p. 307.

§ Cicero ut antea. De Non, p. 321, et ſequent. Swinburne, Vol. I. p. 313, 314, 315. Rollin's Rom. Hiſt. Vol. V. p. 204.

Tyche,

Tyche, or the third city, extended northerly from Achradina to Epipolæ; commencing at the bottom of the port of Trogilus, which forms its ſouth-eaſtern boundary. Eaſt, it was defended by a ſtrong wall; and a wall divided it, on the weſt, from Neapolis. The famous gate of Hexapylon was on the eaſtern wall, and oppoſite to the little town of Leon. Tyche was ornamented by the Gymnaſium; and, though now deſolate, was once filled with inhabitants. It is very elevated, riſing in rapid gradation from the wall of Achradina *.

Neapolis ran nearly parallel, and in the ſame direction with Tyche; terminating on the north, with that quarter, at Epipolæ. On the eaſt, ſouth, and weſt, it was equally protected by a wall. The upper and northern part of Neapolis, as of Tyche, was elevated; its ſouth-weſtern extremity was conſiderably lower. A noble theatre, and the temples of Ceres and Bacchus, were its moſt diſtinguiſhed ornaments †.

Epipolæ was originally a piece of high ground without the city, and afterwards ſo little inhabited that it is not mentioned by Cicero in his deſcription of Syracuſe. The moſt elevated ſituation, and commanding Tyche and Neapolis, it was judiciouſly incloſed by Dionyſius I. who ſurrounded it with a wall of near four miles in extent. Its additional defence was the fortreſs of Labdalon, at its bottom, on the eaſt; and that of Euryalus, at its top, on the north ‡.

The whole extent of the city of Syracuſe, according to Strabo, whoſe account is verified by the concurrent teſtimony of Mr. Swinburne, was equal to twenty-two miles and four furlongs Engliſh meaſure §.

To this brief deſcription of the principal diviſions of this famous city, it is neceſſary to add a few remarks relative to its immediate vicinage, and the waters by which it is ſurrounded.

The *Great Harbour*, or Port, is about five miles in circumference, and forms, at its north-weſtern extremity, the

* Cicero. Rollin. De Non. Swinburne.
† Cicero. Rollin. De Non. Swinburne.
‡ Rollin. Swinburne. De Non.
§ Swinburne, Vol. II. p. 329.

Gulf

Gulf of Daſcon. Lower down, and oppoſite to Ortygia, it is contracted by the promontory and fort of Plemmyrium. The *Little Port*, or Portus Marmoreus, divides Ortygia from the Continent on the eaſt, and waſhes the ſouthern wall of Achradina. The *Bay of Thapſus* runs up behind Achradina on the eaſt, and forms, by the junction of its weſtern extremity with the city, the *Port of Trogilus* *.

At the diſtance of about two miles from Ortygia, and ſomewhat leſs than a mile and a half from Neapolis, (whoſe weſtern wall croſſed the low grounds through which it runs,) the river Anapus empties into the great harbour. This ſtream, which is only twenty-four feet wide, and twelve or fifteen deep at its mouth, flows, in a ſerpentine courſe, through a ſmall extent of country, which, though ſlightly elevated on its ſouth or ſouth-weſtern ſide, on the north and north-weſt conſiſts of an extenſive marſhy plain †.

Between the Anapus and the promontory of Plemmyrium was ſituated the little ſuburb of Olympia, ſurrounding the ſcite of the ancient temple of Jupiter Olympus, built upon an eminence, and bounded on either ſide by the vaſt Lyſimelian marſhes, extending from the head of the great harbour, half covered with water in the vernal months, and exhaling, under a vertical ſun, the moſt unwholeſome and pernicious vapours ‡.

The preceding detail, it is hoped, will neither be found altogether tireſome, nor foreign to the purpoſe of this eſſay. Some part of it might, indeed, have been omitted, as not directly eſſential to the main deſign of the paper; but there would have been leſs unity in the deſcription, and ſome portion of the ſubſequent narration might have been leſs perfectly comprehended.

* Rollin. Swinburne. De Non.

† Rollin. Swinburne. De Non. Thucydides, B. vi. and vii.

‡ Rollin. De Non. Swinburne. Watkin's Travels, Letter xxiv.

The reader will find great aſſiſtance in comprehending diſtinctly every part of the preceding deſcription, from conſulting the plan of Syracuſe in Rollin's Ancient Hiſtory, or that in his Roman Hiſtory, which, though inaccurate in ſome reſpects, I have reaſon to believe the moſt correct.

SECT.

Sect. II.—The peſtilential diſeaſes prevailing in the neighbourhood of Syracuſe, to which the preſent inquiry is limited, occurred at three diſtinct periods: at the three ſieges of that city, by the Athenians and their allies, under Nicias; by the Carthaginians, under Imilcon; and by the Romans, under Marcellus. It is exceedingly to be regretted, that, conſidering the ſingular mortality which diſtinguiſhed each of theſe plagues, the information concerning them is ſo ſcanty and general. But the preciſion which remarkably characteriſes that which remains, renders it of ſufficient importance to deſerve particular attention. It is the purpoſe of the preſent Section to place before the reader, in a ſuccinct narration, the circumſtances which the hiſtorians have preſerved relative to each of theſe intereſting events.

In the ſecond year of the famous conteſt between the Syracuſans and the Athenians, of which Thucydides has left ſo minute and ſo touching an account, the Athenians, whoſe principal encampments were on the eaſt of Syracuſe—at Leon, at Thapſus, at Labdalium, and on the port of Trogilus—undertook the conſtruction of a wall, which extended to Epipolæ, and was deſigned, after croſſing the plain and the marſhes of the Anapus, to reach to the great harbour on the weſt, and incloſe the city. From the completion of this enterpriſe they were prevented by the exertions of the Syracuſans, who carried a diviſion-wall from the city acroſs the marſh, and thus oppoſed an inſurmountable barrier to the progreſs of their beſiegers. To promote his deſign, of the ſucceſs of which he now began to entertain doubts, and to enable him more effectually to annoy the enemy by water, the Athenian general fortified and tranſported his army to Plemmyrium. From this time the fortune of the Athenians changed: their fatigue was great and inceſſant; every trifling ſucceſs was ſucceeded by ſome ſad reverſe; relief was diſtant and uncertain; and they became a prey to deſpondency, not a little heightened by a ſenſe of the injuſtice of their cauſe, their exemplary humiliation, and the unexpected ſuperiority of their antagoniſts. Under theſe circumſtances, in the autumnal ſeaſon, and "encamped on marſhy and

unwholeſome ground *," a ſituation " always unhealthy for an army, and eſpecially in this ſeaſon of the year †," a peſtilential diſeaſe ſhewed itſelf in the camp, extending with the progreſs of the ſeaſon, and the continued expoſure of the ſoldiery, or, as Plutarch expreſſes himſelf, by contagion; till, haraſſed by the enemy on one part, and worn down by fatigue and ſickneſs on the other, the Athenians were driven to the ſad neceſſity of attempting a forced and ſecret retreat, with the dereliction of their camp, their wounded, and their ſick. The conſummate eloquence of Thucydides alone is adequate to the deſcription of this ſcene of horrors.

Of the numbers who periſhed by this peſtilence, and of its particular ſymptoms, no record is tranſmitted down to us. The event of this ſiege is known. The Athenians were ſurrounded on their retreat, and defeated with immenſe deſtruction, and under the moſt melancholy circumſtances of diſtreſs: their general, worthy of a better fate, was cruelly put to death; and the greater part of thoſe who were made priſoners periſhed in Syracuſe, the victims of diſeaſes induced by exceſſive labour and unwholeſome food. A few (in ſuch honour was poeſy among the ancients,) were emancipated by the recollection and recitation of even a ſingle verſe of the pathetic tragedies of Euripides ‡.

At a ſubſequent period, and during the war between the Carthaginians and Dionyſius the elder, Himilco, or Imilcon, after ſeveral ſucceſsful enterpriſes in other parts of Sicily, marched againſt Syracuſe. He invaded it with an army of 300,000 foot and 3000 horſe; while the Carthaginian fleet, of 200 ſhips, under the command of Mago, followed by 500 barks, entered the great port in a triumphal manner, and laden with the ſpoils of the ravaged cities of the iſland. Imilcon pitched his tent in the very temple of Jupiter, then ſtanding at Olympia; and his army encamped around him

* Smith's Thucydides, Vol. II. p. 236.

† Plutarch, art. Nicias.

‡ *Ibid.* — For the particular hiſtory of the ſiege of Syracuſe, the reader is referred, generally, to Thucydides, B. vi. and vii.; and to the Life of Nicias, by Plutarch.

In

In this ſituation, "an eminence between two moraſſes, highly favourable for a camp, and for rendering it impregnable," he continued thirty days, laying waſte the country on all ſides, plundering the temples, and demoliſhing the tombs of the kings, and in vain offering battle to the Syracuſans, who had not the courage to attack or oppoſe him. Nature, more powerful than themſelves, arreſted the progreſs of that fate which threatened to overwhelm them. "It was now in the midſt of ſummer, and the heat this year was exceſſive." While Imilcon continued at Olympia, ſpreading devaſtation around him, and anticipated a ſignal revenge upon the hoſtile Syracuſans, a peſtilential malady ſhewed itſelf in his camp. It appeared firſt among his auxiliaries, the Africans, who, forced into the ſervice by fear of the Carthaginians, whom they hated, were probably leaſt carefully accommodated. From them it ſoon extended, by the increaſing virulence and activity of the exciting cauſe, throughout the army. Neither care nor medicine afforded relief. At firſt the ſick received ſome aſſiſtance from the well; but the peſtilence and the mortality multiplied themſelves ſo rapidly in every direction that this ſoon became impoſſible, nor were there men to be found for the performance of the rites of ſepulture to the accumulating dead. "Violent dyſenteries, raging fevers, burning entrails, and acute pains in every part of the body," were the uſual ſymptoms of this terrible diſeaſe. Some were even ſeized with madneſs, and in their phrenſy attacked and endeavoured to deſtroy all that were expoſed to their aſſaults. Meantime the Syracuſans, in health and ſafety in their elevated, dry, and airy city, watched the progreſs of the peſtilence among their enemies, and, profiting by their forlorn condition, captured or involved in flames their mighty fleet; and, ſtorming their camp, impregnable to the greateſt force when defended by even a ſmall body of healthy troops, defeated them with exemplary deſtruction. Thus beſet, enfeebled by diſeaſe and humbled by defeat, the proud Imilcon, who, a ſhort time before, held the conqueſt, not only of Syracuſe, but of Sicily, too cheap and eaſy a victory for ſo formidable an hoſt, was reduced to purchaſe his ſafety

for 300 talents, and fled, leaving unburied the carcaſes of 150,000 of his ſoldiery *.

The hiſtory of the laſt of thoſe peſtilential diſeaſes, which are particularly ſelected for preſent conſideration, deſerves a longer and more attentive examination. Furniſhing excellent illuſtration of ſeveral important points in the hiſtory of theſe diſorders, the reader will doubtleſs excuſe ſuch preliminary narration as may conduce to place it more fully before him.

The Roman army, under the command of Marcellus, decamped from their ſtation at Leontium, the modern Lentini, and, arriving at Syracuſe, encamped at Olympia. After an unſucceſsful attempt at negociation, the city was beſieged in due form; and the attack commenced on Hexapylon by land, and on Achradina by water. But the Roman general was not ſucceſsful at Syracuſe, as he had been at Leontium: all his exertions were rendered nugatory by the talents of a ſingle citizen; and the genius of Archimedes triumphed over the power of Rome.

* Rollin's Ancient Hiſtory, Vol. I. p 302, 30 , 04 De Non's Travels, p. 358, 359, &c. Diodorus Siculus, Lib. xiv. p. 279—295.

The Carthaginians ſeem to have been deſtined to owe the defeat of many of their moſt promiſing enterpriſes in Sicily to peſtilential diſeaſes. In a former part of the ſame war, whoſe unhappy termination has been deſcribed above, Hannibal, the predeceſſor of Imilcon, laid ſiege to the city of Agrigentum (Girgenti), in the ſouth-weſtern part of Sicily. For the purpoſe of raiſing a wall without, which ſhould overlook and command the city walls, he collected all the materials within his reach, and, among the reſt, deſtroyed, and converted to this uſe, the tombs ſtanding round a city very ancient and populous, and then containing 200,000 inhabitants. From the uncovering and diſturbing of ſo many dead bodies aroſe a terrible peſtilence, which carried off immenſe numbers of the Carthaginians, and the general himſelf. Afflicted at this dreadful mortality, the beſiegers attributed it, with the ſuperſtition of the age, to the vengeance of the gods, incenſed againſt them for violating the repoſe of the dead. The healthineſs of the ſituation, the ſeaſon, and the thorough appointment and ſupplies of the Carthaginian army, leave no room to doubt as to the real cauſe of the ſickneſs, which gradually diſappeared: but two remarks, of ſome importance, are ſuggeſted by it. 1. The folly of modern nations, eſpecially in warm climates, in ſuffering the interment of the dead within their cities. 2. The wiſdom of ſome ancient nations, in having a *dead*, as well as a *living* town.

Neceſſitated

Neceſſitated to defer the further operations of the ſiege, the conſul directed his arms againſt ſeveral other Sicilian cities, and ſtruck his enemies with terror by his brilliant achievements. About the ſame time the Carthaginian general, Himilco, arrived at Agrigentum, to the aid of the Sicilians, with an army of 25,000 foot, 3000 horſe, and twelve elephants. Hippocrates, one of the Syracuſan leaders, with a part of the Syracuſan troops, marched out to meet him; Epicides having been left, with the remainder, for the defence of the city. After ſome ſkirmiſhes with the Sicilian army, in which he was fortunate, Marcellus returned to Syracuſe, whither he was ſoon followed by Himilco and Hippocrates, who, having formed a junction, fixed their camp at the river Anapus, about eight miles from the city. Nothing of importance occurred while the armies lay near each other. The combined chiefs ſoon drew off their forces; the Carthaginian general took up his winter reſidence at Agrigentum, and the Sicilian at Murgantia. Marcellus, who had been for ſome time buſied in the interior, now again returning, appointed Criſpinus to the command of the ancient camp at Olympia, and built and fortified a camp at Leon, on the eaſtern ſide of Syracuſe, for himſelf.

The Romans commenced their operations early in the ſpring. Some of them having gained an entrance into Syracuſe in the night, by ſtratagem, the gate at Hexapylon was broken open, and Marcellus entering, ſecured poſſeſſion of Epipolæ before any effectual reſiſtance could be attempted by Epicides. Tyche and Neapolis ſurrendered at diſcretion; the fortreſs of Euryalus ſpeedily ſubmitted; and the conſul diſpoſed this part of his army againſt Achradina, in three places, hoping to carry it by attack, or ſubdue it by famine.

While theſe preparations were going forward on the part of the Romans, Himilco and Hippocrates ſuddenly returned to Syracuſe, and encamped on the great harbour. From hence they attacked Criſpinus in the ancient camp of the Romans, while Epicides, in concert, ſallied out from the city upon the poſts of Marcellus. Neither of theſe aſſaults ſucceeded. Criſpinus repulſed the allies, and purſued them triumphantly

umphantly to their ſtation, at the ſame time that Marcellus compelled Epicides to take refuge within the walls of Achradina. After this, both of the Roman commanders ſtrengthened their encampments.

Such was the progreſs and ſtate of the war when a peſtilence, common to both armies, appeared both in the camp of the allies and of Criſpinus, and naturally diverted their attention from hoſtile operations: for the autumnal ſeaſon, their unhealthy ſituation, and the heat, (much more intolerable without than in the city,) affected almoſt every perſon in either camp. At firſt they became ſick, and died, ſimply from the effects of the ſeaſon and the unhealthineſs of their ſituation: afterwards, ſays the hiſtorian, the care and contact of the ſick ſpread the diſeaſe; ſo that thoſe who were attacked with it periſhed, neglected and forſaken, or their attendants fell victims to their humanity. Deaths and funerals paſſed before the view in rapid ſucceſſion, and day and night reſounded with lamentations. At length theſe ſcenes of calamity became ſo familiar, that they not only neither wept, nor lamented the dead, but even ceaſed to remove, or yet to inter them. Their lifeleſs bodies lay extended in heaps, in the very ſight of thoſe who expected a ſimilar fate; while the dead infected the ſick, and the ſick thoſe who were in health, as well with fear as with the corruption and peſtiferous exhalations from the bodies: ſo that, impatient of life, and deſirous rather to fall by the ſword, ſome ſingly invaded the ſtations of the enemy. The plague raged with far greater violence in the Carthaginian than in the Roman camp; for the Romans, from long reſidence in the vicinity of Syracuſe, had become accuſtomed to the air and water. As ſoon as the Sicilian auxiliaries of the Carthaginians perceived that the diſeaſe ſpread from the inſalubrity of the place, they betook themſelves to their neareſt cities; but the Carthaginians, admitted into no city, together with their generals, Himilco and Hippocrates, totally periſhed. Marcellus, obſerving how faſt the diſorder increaſed, drew the remainde of his troops into the city, where, under ſhelter, and in the ſhade of the houſes, they regained health and vigour, notwithſtanding

withſtanding many of the Roman army were carried off by the ſame peſtilence *.

Thus the Lyſimilian marſhes triumphed over a third army; and a number not leſs than 30,000 were added to thoſe who had fallen of the ſoldiery of Nicias and of Imilcon. The practical conſideration of theſe events belongs to the ſucceeding Section.

Sect. III.—The ſimplicity and uniformity of the preceding ſtatements and narrative, preclude the neceſſity for laborious inveſtigation and argument. The deductions to be made are unavoidably ſimple and uniform; but their force and tendency may be more diſtinctly perceived from a methodical arrangement.

1. Climate and ſeaſon.—The climate of Sicily in general, and particularly of the city of Syracuſe, is remarkably pleaſant during a conſiderable part of the year. But the ſummers are exceſſively ſultry; and this heat, operating on marſhy ſituations and ſtagnant waters, never fails to engender malignant fevers. To theſe they are peculiarly liable in the ſummer and autumnal months. The peſtilential diſeaſes which proved ſo fatal to the armies of Nicias and Imilcon, and to thoſe of the Carthaginians and the Romans, occurred either in the autumn or in the height of ſummer; and, in every inſtance, in the immediate vicinity of Syracuſe.

2. Situation.—The camp of Nicias was near or at Plemmyrium; of Imilcon, at Olympia; of Criſpinus, at Olympia alſo; and of Himilco and Hippocrates, at the head of the great harbour. The whole of this territory is a vaſt marſh, but partially covered with water even in ſpring, extremely offenſive in ſummer, and noted, from time immemorial, for its inſalubrity.

3. Condition of the armies.—The Athenian army was in conſtant action, and conſtantly unſucceſsful. The army of Imilcon was in perpetual movement, but fortunate, and full of courage. The adverſe armies, at the third ſiege, were in a ſtate of inaction. Theſe are certainly conſiderable dif-

* Tit. Liv. typis Barbou, 1775. Tom. III. Lib. xxiv. & xxv. p. 303—403.

ferences;

ferences: why ſhould they not modify the event? It may be doubted whether any moral cauſe would be ſufficient to protect, for a long period, an unaccuſtomed reſident in a marſhy ſituation from the uſual conſequences. Fatigue and deſpondency are certainly favourable to the acceſs of diſeaſe; and inaction, particularly in camps, is generally admitted to be ſcarcely leſs ſo. The uncommon heat of the year of Imilcon's irruption into Sicily, may be a ſatisfactory explanation why his ſucceſsful troops were ſo readily affected by the unwholeſomeneſs of his camp.

4. Progreſs of the diſeaſe.—The ſick, in the army of Nicias, were not numerous at firſt, but the number gradually increaſed. This is attributed, by Plutarch, to contagion; but Thucydides, a cotemporary and more ſagacious hiſtorian, expreſſes no ſuch opinion, nor does it ſeem probable. The growing deſperation of the Athenian affairs, and the tendency of the ſeaſon to augment the extent and heighten the virulence of marſh exhalations, ſufficiently account for the increaſing prediſpoſition to ſickneſs, and for its more general prevalence in their army.

The ſame reaſoning will apply, generally, to the caſe of Imilcon's army; but there were, in this inſtance, additional cauſes for the production of ſuch immenſe mortality. The commencement of this peſtilence was among his African auxiliaries. The circumſtances of their impreſſment into the ſervice, and of the contempt in which they were held by the Carthaginians, render it probable that leſs care was taken to accommodate them than the others. Some peculiar habits of body, modes of life, or varieties of native climate, might influence this event; or they might have been previouſly ſubjected to greater fatigue. Be this as it may, the ſickneſs ſoon became univerſal, and ſo mortal that the dead lay unburied. In a ſituation like this, there is no reaſon to believe that much attention was at any time paid to the neceſſary duties of cleanlineſs. When the ſick could find no attendants, and the dead none to inter them, it requires but little ſagacity to infer that no means were uſed for the removal of excrementitious matters. And as a large proportion of this army were afflicted with dyſentery, and the number of pu-

trefying carcaſes muſt have been immenſe, and as the limits of the camp were not very extenſive, and they were now forely preſſed by a vigorous enemy, incapable themſelves of reſiſtance, and ſuddenly precipitated from triumph to inevitable defeat and ſhame, we ſhall find no difficulty in aſſigning cauſes ſufficient and ſatisfactory for the extraordinary fatality of this peſtilence.

The third plague commenced at the ſame time in both the Carthaginian and Roman camps. At firſt, as in the former inſtance, the ſick were attended to; but the number ſoon became ſo great as to render this impoſſible. In this caſe, as in the other, the dead were left unburied, and to putrefy; and in this caſe, as in that, there is no reaſon to doubt but that the cauſes and fatality of the diſeaſes were in conſequence exceedingly augmented. It may reaſonably be doubted, however, whether the hiſtorian is accurate in aſcribing any part of the ſpread of the peſtilence to contact of the ſick. The belief of this doctrine has been of long duration and extenſive prevalence; but late obſervations do not ſeem to countenance its validity. To eſtabliſh it, would inevitably annihilate a diſtinction much inſiſted on by the advocates for the importation of peſtilential diſeaſes into the United States; but this argument is not wanted for that purpoſe. It is more probable that the peſtilence, in this inſtance, as in that preceding, was rendered more general by the increaſing quantity and concentration of marſh effluvia; by the excrementitious matters in the camps, ſuffered to aſſume, unremoved, a putreſcent action; and by noxious exhalations from putrefying bodies. There are no facts which authoriſe the opinion that a ſpecific matter emanates from any part of the body, during peſtilential or any other fevers properly ſo called, capable of generating a ſimilar or a febrile diſeaſe. The moſt that we may venture to infer from all the facts known to us, is, that the perſpirable matter, like any other animal ſubſtance, when ſeparated from the living body, or deprived of life, is liable, under certain circumſtances of temperature and moiſture, to undergo a putrefactive fermentation, or to go through ſuch changes as ſhall adapt it for the production of febrile diſeaſes; and this,

as well when ſuffered to lie on the ſkin, as when elſewhere, or otherwiſe diſpoſed of. With theſe limitations, and in this view of the queſtion, there need be no objection to the teſtimony of Livy; nor have we any ground, from a knowledge of the ſymptoms of the plague he deſcribes, to oppoſe or fortify his narration.

5. Mortality of the peſtilence.—Of the mortality of the ſickneſs which affected the army under Nicias, we only know that it was conſiderable. Our knowledge is not much more definite reſpecting the number of the Carthaginians who periſhed of the army of Imilcon. There is reaſon, however, to believe that it far exceeded that of the Athenians, and probably it was not leſs than 100,000 men. For it muſt be recollected, that many had been regularly interred in the firſt of the plague, and that all the ſick and wounded were left behind when their general ingloriouſly fled from Sicily. Nor is it probable that a number greatly exceeding 50,000 fell in the battle which preceded his flight. In the laſt caſe, our computation may be nearer the truth. The army with which Himilco invaded Syracuſe amounted to 28,000, excluſive of his Sicilian auxiliaries, and of the troops from that city which joined him under the command of Hippocrates. The defeat of this laſt, by Marcellus, the preceding year; the ſeceſſion of the Sicilians; and his own loſſes in the various actions in which he had been previouſly engaged, had no doubt reduced his army below its original ſtrength. But, when it is remembered that all this army periſhed, and that a leſs, but ſtill a great number of the Roman army alſo were deſtroyed by the plague, there is reaſon to eſtimate the whole loſs as exceeding 30,000. The entire number of men ſacrificed to this unhealthy ſituation, in three ſucceſſive periods, could not be leſs than 150,000: a moſt melancholy illuſtration of the influence of climate, ſeaſon, and ſoil, on the health of mankind; and an example of the activity of local cauſes in producing diſeaſe, compared with which, all the yellow fevers of the United States, whether originating here, or imported from abroad, ſcarcely deſerve to be remembered.

6. Symptoms and nature of the peſtilence.—On this point our information is leſs complete than on any other.

The ſickneſs in the Athenian army is ſimply called a fever, ſaid to be contagious by Plutarch, but probably not ſo. Livy deſcribes no ſymptoms of the plague recorded by him. Diodorus Siculus is ſomewhat more particular; but only ſo far as to mark the variety of form in which the ſoldiers of Imilcon were attacked by the diſeaſe. In ſome, in the ſhape of enteritis, or inflammation of the bowels; in others, of dyſentery, ſometimes invading as a violent fever, with acute pains in every part of the body, and ſometimes with madneſs or delirium. Yet, even theſe ſcanty particulars are of importance, as they ſerve to identify the diſeaſe, and to proclaim its ſtrict affinity to thoſe which are the common offſpring of ſuch ſituations as that in which the army were encamped, and thoſe which have ſpread ſo much apprehenſion, and excited ſo much diſcuſſion in our own country.

7. Ceſſation of the peſtilence.—Another circumſtance which characteriſes the diſeaſe under conſideration, is the manner in which it was extinguiſhed. So long as Nicias remained in his camp, he ſaw his men conſtantly dying around him by ſickneſs. His removal, notwithſtanding the ſubſequent calamities which befel him, appears to have thoroughly delivered his army from fevers. The ſurvivors were employed as ſlaves in Syracuſe; which could ſcarcely have happened had they been ſick, or had the Syracuſans dreaded the introduction of a contagious diſeaſe by their means.—Imilcon preſerved the reſidue of his army only by flight: there was no other hope for their ſafety; and, after his return to Carthage, he exclaimed, in the bitterneſs of his grief, that the plague, not the enemy, had conquered him! But neither then nor before did the dry and airy city of Syracuſe ſuffer from any ſickneſs; nor did the fear of contagion prevent the Syracuſans from repeatedly attacking the Athenian and Carthaginian camps. Their minds were probably unwarped by the bias of ſyſtem; and they diſcerned, in the poſition of their enemies, the true cauſe of their misfortunes.—The facts related by Livy are ſingularly preciſe and important. In the firſt place, neither the citizens of Syracuſe, nor the army of Epicides, which had poſſeſſion of Ortygia and Achradina,

nor that part of the Roman army which, under the command of Marcellus, was ſtationed in the elevated quarters of Tyche, Neapolis, and Epipolæ, were in the leaſt affected by the plague. But this calamity was limited to the Romans under Criſpinus, in the ancient camp at Olympia; and to the allies, who were encamped at the head of the great port. Criſpinus and his troops, who had lived at Olympia a conſiderable part of the two preceding years, or at leaſt had remained in the vicinity of Syracuſe, and had become in a degree habituated to the air and water, ſuffered leſs ſeverely than their enemies; and, when the ſickneſs became general among them, recruited, and ceaſed to be taken ſick, in conſequence of a removal to the high grounds of the city, occupied by their countrymen. The natives of the iſland, ſeeing the danger to which they were expoſed, took refuge in their own cities, and eſcaped the diſeaſe; but the Carthaginians, without any place of refuge, and entirely unaccuſtomed to their ſituation, totally periſhed.

8. On the whole, then, it appears that the mortality, in every inſtance, occurring in the armies near Syracuſe, originated entirely from local cauſes: that there is no reaſon to ſuſpect that this mortality was heightened by contagion, in the uſual acceptation of that term: that the ſymptoms, ſo far as we have any account of them, were ſimilar to thoſe which occur, under ſimilar circumſtances, in the Eaſt and Weſt Indies, in our own country, and in every part of the world: and that, as no difference is recorded as exiſting between theſe great epidemics or endemics before and after the cauſes were increaſed by animal putrefaction, the advocates for the foreign derivation of peſtilential diſeaſes muſt relinquiſh the diſtinction between fevers from animal and vegetable putrefaction; or, if they maintain the contagious quality of the former, muſt admit, on equal evidence, the ſame quality as characteriſing the latter. To this it may be added, that, as it was the univerſal practice of the ancients to deſignate any and every wide-waſting diſeaſe by the name of plague, without any ſpecial reference to its peculiar ſymptoms, ſo there is an equal propriety in our conferring the ſame appellation on

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our dyſenteries and fevers; nor do theſe leſs deſerve the title than the fevers of Smyrna, Cairo, and Conſtantinople, which reſemble them in their origin, their varieties of prevalence and degree, their appearance and diſappearance, and which would, in all probability, yield to the ſame regimen and remedies.

VIII. *Obſervations on the Art of Scowering different Kinds of Stuff. By* C. Chaptal *.

THIS art ſuppoſes, 1ſt, a knowledge of the different ſubſtances capable of ſtaining any kind of cloth; 2d, of the ſubſtances to which recourſe muſt be had in order to make thoſe depoſited on the ſtuff to diſappear; 3d, a knowledge of the effects produced on colours by thoſe re-agents which it may be neceſſary to employ to deſtroy ſtains; 4th, a knowledge of the manner in which the cloth is affected by thoſe re-agents; 5th, of the art of reſtoring a colour changed or faded.—Of thoſe bodies which occaſion ſpots on different kinds of cloth, ſome are eaſily diſtinguiſhed by their appearance, ſuch as greaſy ſubſtances; but others have more complex effects, ſuch as acids, alkalies, perſpired matter, fruits, urine, &c. Acids redden black, fawn, violet and puce-colour, and every ſhade communicated with orchilla-weed, iron, aſtringents, and every blue except indigo and pruſſian blue. They render the yellows paler, except that of arnatto, which they change into orange.

Alkalies change to violet the reds produced by Brazil wood, logwood, and cochineal. They render the greens on woollen cloth yelloviſh, make yellow browniſh, and change the yellow produced by arnatto to aurora. Perſpired matter produces the ſame effects as alkalies.

When the ſpots are produced by ſimple bodies on ſtuffs, it is eaſy to remove them by the means already known. Greaſy ſubſtances are removed by alkalies, ſoaps, the yolk of eggs, fat earths; oxyds of iron, by the nitric and oxalic acids; acids by alkalies, and reciprocally. Stains of fruit on white

* From the *Bulletin des Sciences*, Vol. II. No. 4.

ſtuffs

ſtuffs may be removed by the ſulphurous acid, and ſtill better by the oxygenated muriatic acid. But when the ſpots are of a complex kind, it will be neceſſary to employ ſeveral means in ſucceſſion. Thus: to deſtroy the ſtain of coom from carriage-wheels, after the greaſe has been diſſolved the oxyd of iron may be removed by the oxalic acid.

As colours are often changed by re-agents, it will be neceſſary, in order to reſtore them, that the ſcowerer ſhould poſſeſs a thorough knowledge of the art of dyeing, and how to modify the means according to circumſtances. This becomes the more difficult when it is neceſſary to re-produce a colour ſimilar to that of the reſt of the ſtuff, to apply that colour only in one place, and often to reſtore the mordant by which it was fixed, and which has been deſtroyed, or even the firſt tint which gave the colour its intenſity. It may be readily conceived that the means to be employed muſt depend on the nature of the colour and the ingredients by which it was produced; for it is known that the ſame colour may be obtained from very different bodies. Thus, after an alkali has been employed to deſtroy an acid ſpot on browns, violets, blues, poppies, &c. the yellow ſpot which remains may be made to diſappear by a ſolution of tin; a ſolution of ſulphat of iron reſtores the colour to brown ſtuffs which have been galled; acids reſtore to their former ſplendour yellows which have been rendered duſky or brown by alkalies; blacks produced by logwood become red by acids; alkalies change theſe red ſpots to yellow, and a little of the aſtringent principle makes them again become black. A ſolution of one part of indigo in four parts of ſulphuric acid, diluted with a ſufficient quantity of water, may be employed with ſucceſs to revive the blue colour of cotton or wool which has been changed. Scarlet may be revived by means of cochineal and a ſolution of the muriat of tin, &c.

The choice of re-agents is not a matter of indifference. Vegetable acids are preferable; the ſulphurous acid however may be employed for ſtains occaſioned by fruit: it does not change the blue of ſilk nor colours produced by aſtringents: it does not degrade the yellow of cotton. Ammonia ſucceeds better than fixed alkalies in removing ſpots produced by acids.

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It is employed in vapour; its action is ſpeedy, and ſeldom alters the colour.

The means of removing greaſy ſpots are well known. This effect is produced by alkalies, fullers' earth, volatile oils diſſolved in alcohol, a heat proper for volatilizing greaſe, &c. Spots occaſioned by ink, ruſt, or iron-mould of any kind, and all thoſe produced by the yellow oxyd of iron, are removed by the oxalic acid: the colour may be reſtored by alkalies, or a ſolution of the muriat of tin. Theſe ſpots may be removed alſo by the oxygenated muriatic acid, when they are on white ſtuffs or paper.

The action of alkalies, and that of perſpired matter, are the ſame: their ſpots may be effaced by acids, or even by a weak ſolution of the muriat of tin. When theſe ſpots ariſe from ſeveral unknown cauſes, in order to deſtroy them recourſe muſt be had to *polychreſt* compoſitions. The following may be conſidered as one of the moſt efficacious. Diſſolve white ſoap in alcohol, and mix this ſolution with the yolks of from four to ſix eggs: add gradually eſſence of turpentine; and incorporate with the whole ſome fullers' earth, in ſuch a manner as to form balls of a ſuitable conſiſtence. Moiſten the ſpot; and, having rubbed it with theſe balls, the ſpot will be removed by waſhing the ſtuff. All ſpots, except iron-mould and ink, may be removed in this manner.

Waſhing deſtroys the luſtre, and leaves a tarniſhed place diſagreeable to the eye; but the luſtre may be reſtored by drawing over the waſhed place, and in the direction of the pile, a bruſh moiſtened in water impregnated with a little gum. You may then apply a ſheet of paper, or a piece of cloth, and a conſiderable weight, under which the cloth muſt be left to dry.

IX. *Deſcrip-*

IX. *Description of some Improvements invented by Mr.* JAMES BURNS, *of Glasgow, applicable to Fire-Grates, Stoves, &c. by which Rooms and Halls of every Description may be heated more speedily and effectually, and with a smaller Waste of Fuel, than by the Methods now in Use*.*

THE principal merit of Mr. Burns's improvements, and it is not a trivial one, consists in his contriving to maintain the combustion of the fuel in *open* stoves or grates, without employing for that purpose the air of the room in which they are placed. The heat liberated and thrown out into the apartment is thus prevented from being unnecessarily wasted and hurried up the chimney, as is unavoidably the case in the usual method, where the combustion is maintained by air from the apartment, the waste of which is supplied by exterior cold air that comes pouring in at the bottom of the doors and the sides of the windows, thereby undoing a great part of the effect produced by the fire.

To prevent this waste, the air that maintains the fire in Mr. Burns's grates, or in others to which his improvements are applied, should be brought through a tube from the outside of the house †, or be made to pass from the outside of the house, between two of the joists, where the floors and ceilings are found enough to allow this, so as to be brought to the bottom bars of the grate, without having any communication with the interior air of the room, while at the same time the grates, and parts connected with them, should be so constructed, that, when the fire is not wished to be supplied with cold air from the outside of the house, the passage may be shut, more or less perfectly, by means of a valve, a small door, a cock, or any similar contrivance. When convenience does not admit of the air-tube being carried to the

* Mr. Burns has lately obtained a patent for these improvements.

† The same principle had been already successfully applied to *close* furnaces: Mr. Howard's, of which we gave an account in our last Number, furnishes an example. But its application to common open fires is a new idea, and will probably produce an entire revolution in the construction of grates for common use.

outside

outſide of the houſe, if carried to a cellar, larder, ſtair-caſe, or any lumber-room, the ſame end will be gained; with this further advantage, that ſuch cellar, larder, &c. will thereby be well ventilated, and prevented from acquiring or retaining any unhealthy or diſagreeable ſmells.

The form of the grates may be varied indefinitely; but the one we are about to deſcribe anſwers the purpoſe ſo well, and is at the ſame time capable of being made, by the aid of a good architect, ſo highly ornamental to any apartment, that our readers will fully comprehend the facility with which it may be accommodated to circumſtances.

Fig. 1, (ſee Plate V & VI.) repreſents a grate of the improved conſtruction, and Fig. 2 is a ſection of it. The air that maintains the combuſtion is ſupplied through the pipe or tube A, (Fig. 2 and Fig. 4,) or from between the under-ceiling and floor as before-mentioned, from which it paſſes up by the back or ſide of a drawer *B*, Fig. 2, according as it is moſt convenient to bring it up by the one or the other; the back however is preferred, where convenience admits of it. The front of this drawer, in its place, is ſeen in Fig. 1, at C. The intention of this drawer is to receive the aſhes that fall from the bottom bars of the grate, ſections of which are repreſented at DD, Fig. 2. The aſhes, as they fall from the bars, ſtrike againſt the ſides of the internal cavity E, and then are carried by their own gravity through the narrow part F, and fall into the drawer. This part of the conſtruction may be eaſily underſtood by conceiving it to be an hollow vaſe, as it actually is, with a drawer in its pedeſtal or baſement, and having a grate over it, on which the fire reſts. The valve, door, cock, or other contrivance by which the external air is admitted or excluded, may be either in the neck F of the vaſe, or in the air-tube, or in the drawer B or C, or, which is preferable, in the cavity behind the drawer, the pedeſtal or baſement of the vaſe being made large enough to admit of ſuch cavity. Various conſtructions may be reſorted to; but that repreſented in Fig. 3, (which is a ground-plan of a proper chimney for ſuch a grate, and in which a bird's-eye view of one half of the grate may be ſeen in its place, while the other half repre-

fents an horizontal fection of the baſement or pedeſtal, which is ſuppoſed to be open, ſhewing the drawer in its place, with the cavity behind it,) will be found to anſwer every purpoſe. The cavity G is ſeparated from the place in which the drawer is put, by a partition (beſt made of plate or caſt-iron) paſſing from H to H, in which there is an opening I, with a cover K. To the cover K is attached a ſmall bar L, worked by another ſhorter bar M, to which it is attached by any ſimple joint. The bar M is faſtened into an upright pivot, the top of which comes up through the pedeſtal at one of its corners or any other convenient ſpot, and is furniſhed with a button to be laid hold of by the finger and thumb, or with a top fitted to a key, by the turning of which the cover K is made to ſhut or open the aperture I, and conſequently to prevent or facilitate the paſſage of the air from the hole at G, which repreſents the internal mouth of the air-tube, the other end of which is on the outſide of the houſe, or in any cellar or other apartment, as before deſcribed. When the air has paſſed through the aperture I, it finds no difficulty in paſſing on to the bottom of the grate, that back or ſide of the drawer next which the aperture is being made low to allow it to flow in freely.

The grates are recommended to be made of a circular or elliptical form, as being moſt convenient, where the fence or ſcreen, to be immediately deſcribed, is wiſhed to be applied along with Mr. Burns's other improvements. The fence is intended to prevent thoſe dreadful accidents, which too frequently occur, of women's and children's clothes being ſet on fire by ſparks from the grate. When it is wiſhed to be adopted, the inſide of the chimney, where the grate is to ſtand, had beſt be a ſemi-cylinder, or nearly ſo, (as repreſented in Fig. 3,) with a lining or cover *aaa*, beſt made of metal, at ſuch a diſtance from the ſemi-cylindrical wall, NNNN, as to give ſufficient room for allowing the ſafeguard or fence to be ſlid round into it when the fire is wiſhed to be left open, when freſh fuel is to be added, or when the drawer with the aſhes is to be removed.

The fence is a frame-work of metal, which, when filled up with glaſs or with wire-work, forms a portion of a cylin-

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der anſwerable to the curvature of the ſpace between the back of the chimney and the lining above-mentioned, made in one or more pieces, and moving in a circular groove, *bbb*, in or upon the hearth, which ſerves to conduct it into its place behind the grate, when the fire-place is wiſhed to be left open, as before mentioned. The top of the front of the opening of the chimney O, Fig. 4, projects in a circular form, or is furniſhed with an added projection, made of metal, marble, or any other fit material; but in either caſe the projection is furniſhed with a circular groove on its under ſurface, of the ſame radius with the one in, or planted on the hearth, for the purpoſe of receiving the upper part of the frame-work of the fence or ſafeguard, which frame-work may be filled up with glaſs, either plain or bent, and either colourleſs or coloured, or ſtained or painted with figures or deſigns of any kind. By this means complete ſafety is obtained againſt any ſuch accidents as have been alluded to, while at the ſame time the comfort ariſing from the view of a cheerful fire is not prevented by the interpoſition of any opake body; but for nurſeries or the like, where convenience and ſafety is more the object than elegance or luxury, the frame-work may be filled up with wire-work. The fence may be ſeen in its place at P.

Where either the glaſs or the wire-work fence, or both of them, are meant to be applied to ſquare or rectangular chimneys, without the trouble of giving them the ſemi-cylindrical form, the lining to receive the fence or fences may be introduced at the ſides or jambs of ſuch chimneys; or the fence may be made to riſe by means of pulleys into the wall above the opening, or ſlide ſideways into the walls at the ſides of the opening.

Theſe improvements may be introduced together or ſingly, and may be applied to many of the grates now in common uſe.

Beſides the advantages already pointed out as connected with them, they poſſeſs alſo the following.—Any room or apartment may be heated by their means with a much ſmaller quantity of fuel than by any other method yet in uſe: at the ſame time the advantage of ſeeing the fire is not

loft, as in clofe ftoves; for thefe grates have fide as well as bottom bars, which allow the radiant heat and light to be thrown out into the room without any impediment: and in fact large rooms, halls, and the like, which by the ufual methods can hardly be warmed, or made at all comfortable in cold weather, may, by means of thefe improvements, be heated as effectually as the fmalleft apartment; for, when their full effect is wanted to be produced, it is only neceffary to keep the fence in its recefs, that even that portion of heat, which would be kept back by the interpofed glafs or wire-work, may be thrown out into the room, and perform its office. They are alfo an effectual cure for fmoky chimneys, which not only caufe great wafte and deftruction of good furniture, but many difeafes to the inhabitants of houfes plagued with that evil. They caufe a clean fire-fide to be eafily commanded at all times, as hardly any of the duft or afhes fall through the fide bars, almoft the whole paffing through the bottom bars down into the drawer; and any fire lighted in fuch improved grates burns up and becomes lively in a few minutes, without the aid of bellows, and that watchful care which common grates or ftoves require.

X. *On the Chemical Action of different Metals on each other at the common Temperature of the Atmofphere.* *By* C. Fabbroni*.

THAT particular fenfation, firft made known by Sultzer, which is felt on the tongue on bringing two metals into mutual contact, and which would have excited none if they had been applied feparately to that organ, has been ranked among the galvanic phenomena. C. Fabbroni, however, inftead of afcribing thefe effects to an agent almoft unknown, fuch as the electric fire, is of opinion that it depends on a chemical operation, perhaps as the fenfation of tafte itfelf. He has endeavoured to prove the truth of this opinion by a number of obfervations and experiments.

* From *Bulletin des Sciences*, Vol. II. No. 5.

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He had remarked that feveral metals, fuch as mercury, tin, lead, retained their metallic brilliancy as long as they were pure, while compounds of them became foon tarnifhed and oxydated; that the mere contact of two different metals feemed to haften their oxydation; and that, in this manner, the mixture employed for foldering the plates of copper which cover the obfervatory of Florence, had been fpeedily changed into a white oxyde at the outer edge of its contact with the copper, &c. He thinks that, in this cafe, the metals have a reciprocal action on each other; and that this action, more efficacious and more fenfible when the attraction of aggregation of the metals is deftroyed by fufion, exifts no lefs between folid metals when they touch each other.

If the effects, as rapid as thofe of lightning, occafioned on the tongue by two metals brought into contact, have been by fome afcribed to a peculiar fluid not galvanic, or to the electric fluid, it is becaufe they did not recollect that chemical action is exercifed between two bodies with the utmoft rapidity. The figns of electricity which have been fometimes obferved on feparating two metals in contact, are rather the confequences than the caufe of that action; for it is known that the greater part of chemical operations change the electric equilibrium of bodies, and muft confequently give birth to electrical phenomena. Without totally excluding electricity, therefore, from all galvanic facts, C. Fabbroni thinks that this fluid has fome fhare in the fenfation experienced by the tongue from two metals in contact. This action of metals in contact is proved by the following experiment:—

C. Fabbroni put into two glaffes filled with water, pieces of different metals, one in each glafs. In other glaffes he put two pieces of different metals, but kept them from touching by interpofing a plate of glafs. In a third feries of glaffes he alfo put two pieces of different metals, but in contact. In the metals of the two firft feries he obferved no change, while the moft oxydable metals of the third feries were covered with an oxyde, which confiderably increafed in the courfe of a few days, and the metallic pieces even contracted a ftrong adherence. The quantity of the calorie which difengages itfelf in thefe combuftions is too fmall to be meafured, yet the light

light which emanates from it may be feen, if the eye itfelf forms a part of the experiment, by holding a piece of filver in the mouth, and applying a bit of tin to the ball of the eye. After thefe two metals are made to communicate, you fee a feeble but diftinct light, which difappears at the end of a few feconds, becaufe the eye becomes accuftomed to this feeble fenfation; but it may be renewed by drawing the metal over the opake cornea, and then over the tranfparent. The author afcribes to a convulfive fenfation that kind of flafh which fome perfons think they perceive * at the moment of the contact of thefe metals, applied one on the tongue, and the other under it.

To make the oxydation of the two metals take place under water, the prefence of air is alfo neceffary. C. Fabbroni thinks that air is ufeful in this circumftance, to add to the water a fufficient quantity of oxygen to be taken up by the metal, as filver is added to gold in order to perform the operation of quartation. If a piece of filver and a piece of tin be put in contact in water, inclofed in a flafk of flint-glafs hermetically fealed, the tin is oxydated; but the oxyde of the lead of the flint-glafs is decompofed, and the glafs becomes black. Philofophers, who afcribe thefe phenomena to electricity, bring, as a proof of their opinion, that they take place when the metals are connected even by means of a pretty long chain. C. Fabbroni has determined the length of this chain at about 18 or 20 feet. Beyond that diftance thefe phenomena are no longer fenfible; while, on the other hand, the electric fluid is communicated to indefinite diftances.

If the phenomena of the experiment of Sultzer belonged really to electricity, they ought to take place with all metals, whatever may be the relation of thefe metals to each other. C. Fabbroni mentions a great number of thefe combinations which produced no effects; and other combinations, of the fame metals, which produced fenfations very diftinct. Thus, if filver be placed on the eye, and gold on the tongue, making them communicate by means of copper, the fenfation is almoft nothing; but it becomes very evident if the iron touches

* The perception is as real as that induced by drawing the metal over the cornea of the eye. EDIT.

the

the eye, and the filver the tongue, the communication being eſtabliſhed as before with copper*.

In regard to the hydrogen of the decompoſed water, the author thinks that it may alſo be abſorbed by the metal: he even conſiders as an hydro-oxyde of tin, the octaedral cryſtals which he remarked on the ſurfaces of the pieces of tin employed in theſe experiments.—It is clearly ſeen, ſays C. Fabbroni, by the reſults which I obtained from the ſimple contact of two metals in water, that is to ſay, by the oxyde and ſaline cryſtals, that a chemical operation takes place, and that to it we ought to aſcribe the ſenſations experienced on the tongue and by the eye. It appears to me, then, probable, that it is to theſe new compounds, or their elements, that we are indebted for that myſterious ſtimulus which produces the convulſive movements of the animal fibre in a great part at leaſt of the galvanic phenomena.

XI. *On the Art of Hardening Copper.* *By* P. I. Hjelm †.

COPPER, in its pure and perfect ſtate, is exceedingly ſoft and malleable: its toughneſs is then ſo great, that it is exceeded only by gold and iron. When copper is hammered a long time cold, and ſtill more when rolled, it is found, as is well known, ſomewhat harder than before, but it does not thereby acquire that ſtrength which deſerves the name of hardening, or which enables it to make ſufficient reſiſtance to ſtrong impreſſions. By being brought to a bright-red heat in the fire, and ſuddenly quenched in water, copper obtains no perceptible addition to its hardneſs; but, on the contrary, becomes more pliable, and conſequently ſofter than before. If the copper is kept a long time in fuſion, or often fuſed in a ſtrong heat, without any covering of flux or charcoal powder, it becomes brittle and unmalleable, and conſequently

* This is no concluſive argument againſt the effect being electrical: it is taking for granted that the laws and properties of the electric fluid have *all* been aſcertained. Edit.

† From *Tranſactions of the Royal Academy of Sciences at Stockholm for* 1787.

harder:

harder: but theſe properties are ſoon loſt when the copper is melted in contact with carbonaceous matter. If melted copper be poured into water, as is done in the operation of granulating or corning, it does not appear that it acquires any perceptible degree of hardneſs, or ſuch as can be compared to that which is communicated to ſteel by the ſame means.

Conſidering all theſe circumſtances, which have been confirmed by experience, it could not but excite attention to find, by the moſt undoubted teſtimony, that the ancients actually poſſeſſed the art of hardening copper, which they employed for inſtruments of all kinds; ſuch as daggers, ſwords, bows, ſhields, javelins, &c. Though ancient authors often mention theſe weapons, none of them have given us any account of the method of hardening the copper. This deficiency ſome have attempted to ſupply by conjectures, which have given riſe to a variety of experiments, but not one of them was ever attended with ſucceſs. As it was imagined that in hardening copper it would be neceſſary to follow the ſame proceſs as that employed for ſteelifying iron, moſt of their reſearches have been directed to a ſimilar method; and for that purpoſe they have not only preſcribed, for the hardening of copper, ſuch proceſſes as are employed for hardening iron when it is to be converted into ſteel, but even the moſt abſurd methods founded upon theſe proceſſes. Theſe reſearches, however, inſtead of anſwering the intended purpoſe, only tend to ſhew the ignorance of thoſe who think they find in the greateſt abſurdity the moſt important ſecrets, and to involve us in ſtill greater darkneſs.

The art of hardening copper has therefore of late been conſidered as one of thoſe known to the ancients, which were afterwards loſt; and it would no doubt have ſtill remained in that ſtate, had not ſeveral of theſe monuments of our anceſtors been brought to light by ſome fortunate accidents, and the reſpect for their great antiquity been overcome by a deſire for becoming acquainted with their compoſition. When this method, the only certain and deciſive one, was once choſen, it was no great difficulty to diſcover that the whole art did not depend on any proceſs like that employed in regard to iron, but on the addition of a certain quantity of

ſome other metal melted with the copper, by which it was rendered harder than before.

The abbé Mongez firſt wrote a treatiſe on the metallic compoſition of the ancient bells, which he tranſmitted to the Academy of Inſcriptions. He found that this compoſition was in general nearly the ſame as that ſtill uſed for bell-metal, that is, copper and tin. M. Mongez afterwards tranſmitted to M. Dizé the point of an ancient copper dagger, the appearance of which on the fracture ſufficiently ſhewed that it had been fuſed and caſt. When diſſolved in pure nitrous acid, there remained a white powder, found to contain tin, and that which had been diſſolved was merely copper. It evidently appeared, therefore, that the ſuppoſed art of the ancients for hardening copper, was nothing elſe than fuſing it with a certain quantity of tin. This was ſtill farther confirmed when M. Dizé found the ſame component parts in ſome Greek, Roman, and Gallic coins, which he obtained on the ſame occaſion for the purpoſe of examination. The tin in them formed about twenty-four parts in a hundred of the mixture.

In ſupport of this opinion, reſpecting the art of hardening copper among the ancients, I have the honour of laying before the Academy ſome experiments lately made on this ſubject.—Laſt ſummer, Profeſſor A. I. Retzius, of Lund, tranſmitted to me a part of the blade of a two-edged dagger, which, together with ſome ſtone-cutters' chiſſels, were found in a hill of earth on the low lands of Scandinavia, where whole ſwords of ancient workmanſhip have often been found. This fragment, on the outſide, had rather a yellowiſh appearance like braſs, than the red colour of copper; the edge was exceedingly thick, and roundiſh; the fracture ſeemed alſo granulated, which evidently ſhewed that the work had been caſt. When tried by the file, it was not quite ſo hard as the common bell-metal, but ſomewhat harder than common gun-metal. The newly-filed ſurface had a reddiſh-yellow appearance, but ſoon became yellower. When melted by the blow-pipe, this compound metal exhibited no traces of zinc; but it might be readily ſeen, by its appearance, that the greater part of the mixture was copper. The filings were

not in the leaſt attracted by the magnet, which clearly proved that there was no iron in the compoſition. It exhibited as little traces of any other metals, as far as could be judged from the aſcending vapour. In order to aſcertain with what metal the copper had been mixed, twenty-five aſſayer's pounds of the pureſt filings of the above fragment were collected; pure nitrous acid was heated in a glaſs retort, and diluted with diſtilled water; and a few of the filings were thrown into it: when the firſt quantity was diſſolved, a few more were added, and this was continued till the whole quantity was put in. The ſolution was made to boil for a quarter of an hour, and then diluted with a little more diſtilled water, in order that a white powder, which remained undiſſolved, and which I ſuſpected to be calx of tin, might more readily fall to the bottom. After this was done, the bright blue-coloured ſolution, which was found to contain nothing but copper, was carefully poured out, and diſtilled water was poured over the white powder, which was waſhed ſeveral times in the ſame manner, after it had each time remained at reſt long enough to allow it to ſettle, when it was thrown upon filtering paper to be farther waſhed. This white powder, when dry, weighed $5\frac{1}{5}$ aſſayer's pounds, which gave $21\frac{1}{5}$ pounds of tin calx in the whole hundred. Now, tin calx loſes, by being revived, $\frac{1}{4}$, or 25 *per cent.* of its weight; and therefore this $21\frac{1}{5}$ pounds of tin calx muſt have given $16\frac{1}{5}$ pounds of metallic tin in the hundred, which, in an experiment made for the purpoſe, was found to be nearly the caſe, and the regulus obtained was found to be pure. The metallic compound of the before-mentioned dagger blade was made therefore of $83\frac{3}{5}$ copper, and $16\frac{1}{5}$ tin; or, to reckon without fractions, of 84 parts of copper, and 16 parts of tin.

By way of experiment, a compound of this kind, conſiſting of pure copper and pure tin, was fuſed, and a penknife blade was made of it, which was poliſhed and ground in the uſual manner. This blade had all the properties and the external appearance of the fragment of the dagger. Where the edge was thin, it was eaſily turned: it was therefore thought that perhaps, in the dagger before mentioned, the proportion of tin was greater. To aſcertain this point, another compoſition

was

was made, of 20 parts of tin and 80 of copper, and a knife blade was made of it, as before. This was much whiter and harder, but alſo brittler in the ſame proportion, and therefore broke by careleſſneſs in the poliſhing. The edge, however, was ſo ſharp that it could be uſed for making pens; but it did not ſtand long, as notches were formed in it by each cut.

When the tin makes twenty-five hundred parts in the mixture, it becomes rather white than red, but exceedingly brittle. If the addition of tin be increaſed to thirty in the hundred, or more, both theſe properties are increaſed in the ſame proportion, and the compoſition becomes fit for ſpecula. In gun-metal tin makes nine parts, or more, in the hundred: in bronze, 84 parts of copper are mixed with about 16 parts of tin, but a conſiderable portion of zinc or braſs is ſometimes ſubſtituted for the latter. Bell-metal contains in general 76 parts of copper, 19 of tin, and 5 of braſs, or thereabouts.

What has been here ſaid may perhaps be ſufficient to confirm the opinion reſpecting the art employed by the ancients to harden copper, and may furniſh ſome hints for the employment of ſuch compounds in common life. Beſides, we are hereby enabled to appreciate the different opinions entertained on this ſubject. M. Monnet imagined that the copper in ancient times was mixed with arſenic, which rendered it hard. No real objection can be made to this being poſſible; but as long as no ancient implements made of this mixture are found, the above aſſertion may at any rate be doubted, without mentioning other circumſtances which ſeem directly to oppoſe it.

M. Dizé * mentions the addition of iron to copper, as the means of rendering the latter harder; and endeavours to prove that Geoffroy the younger, who firſt drew this concluſion, (from an experiment he made, where copper, mixed with ſixteen parts of iron in the hundred, was found to be equally hard, to have the ſame grain on the fracture, and to be as fit for making cutting inſtruments as the hardened copper of the ancients,) was too precipitate in forming his

* *Journal de Phyſique* 1790, April.

opinion. The queſtion, however, aſſumes a different appearance, when we take into conſideration the experiment as related by Count Caylus, in his work on the Egyptian, Etruſcan, Greek and Roman antiquities*: for it there appears that M. Geoffroy undertook the above-mentioned experiment by the deſire of Count Caylus, who deſcribes various kinds of arrows and javelins of ancient workmanſhip, which, though they had the appearance of copper, were mixed with iron; becauſe filings of them were attracted by the magnet; becauſe the fracture had a different appearance from that of other inſtruments made of the hardened copper of the ancients mixed with tin, and was at the ſame time leſs fuſible. M. Geoffroy thinks it rather remarkable that mankind ſhould ſo early have fallen on the method of uniting copper with iron in an uniform mixture, which even at preſent is conſidered as a difficult proceſs. It is well known that the moſt common copper ore conſiſts of copper and iron mineraliſed with ſulphur, and which is called pyrites of copper; not that the copper makes the greater part of it, but becauſe the copper is of the greateſt value, though the iron ſeems moſt generally to conſtitute the principal component parts. When this ore is ſmelted, the firſt copper obtained, or the ſo called black or raw copper, is neceſſarily rendered impure by a greater or leſs quantity of iron, according as more or leſs care has been employed to ſeparate it during the operation. It is therefore in our power, it is ſaid, not only to obtain copper combined in this manner with as much iron as may be neceſſary, but alſo to caſt all kinds of inſtruments of it, and afterwards to hammer them cold, or to expoſe them to the ſame proceſs of hardening as if they were of pure ſteel. Some aſſert that this method has been attended with complete ſucceſs. Count Caylus tried alſo to harden pure copper by melting; but, inſtead of becoming hard, it was found ſofter and more malleable, which agrees with what has been ſaid in the beginning of this paper.

Without in the leaſt leſſening the credibility of this aſſertion, which ſeems to have great probability in its favour, we

* Recueil d'Antiquités Egyptiennes, Etruſques, Grecques et Romains. Vol. I. p. 238—251.

may

may at leaſt expreſs a wiſh that ſome perſon would make, of copper mixed in this manner with iron, different inſtruments, and then endeavour to ſhew real antique works made of the ſame mixture, in order that they may be compared. Until this be accompliſhed, it will be beſt to adhere to that proceſs which ſeems to have been chiefly followed, and of which indubitable proofs are ſtill in exiſtence.

The hiſtory of the antiquity and uſe of metals in the period to which this queſtion properly alludes, is involved in ſo great darkneſs, that nothing deciſive on the preſent ſubject can be derived from it. This much, however, is certain, that the works of ancient authors ſtill extant ſpeak of gold, ſilver, copper, iron, tin, and lead, as known at the ſame time, and employed for various purpoſes. In regard to copper in particular, it is found more abundantly in a native ſtate than any other metal, and requires nothing farther than ſmelting to be immediately uſed. In proceſs of time mankind would become acquainted with the art of extracting the metal from other ores of copper; not, however, without greater labour, and therefore at firſt none but rich ore could be uſed; and we may with juſtice conclude, that copper was one of the firſt metals worked.

Tin is not found in a native ſtate, but its ore is abundant in certain places, and is eaſily revived, or brought to the metallic ſtate, eſpecially when people are acquainted with the proceſs of ſmelting other metals. The antiquity of tin, therefore, is as well eſtabliſhed as that of the other metals among which it is named. In the time of the Tyrians a conſiderable trade was carried on in this metal, which they brought from the Caſſiterides iſlands, beyond the pillars of Hercules, under which name England is no doubt meant.

Whether the art of mixing theſe two metals together by fuſion was firſt found out by accident, or by experiments made for the purpoſe, it is not to be doubted that caſt works of ſuch a compoſition are mentioned at the ſame period with the ſimple metals. Beſides, the works in bronze of the ancients are a ſufficient proof of their ſkill in combining metals, of their art of modelling after Nature, and of their readineſs

readineſs in caſting. All their inſtruments and edged tools of this kind hitherto found have been caſt, and not hammered; and of this kind, without doubt, were the inſtruments mentioned by Profeſſor Pallas as having been found near the Tſchudi mines. All of them, except a few, conſiſted of a compoſition of copper and tin.

The art of preparing and ſeparating iron may have been diſcovered as early as that of preparing other metals. As this art, however, requires a greater degree of dexterity, it was doubtleſs leſs common at firſt, and muſt have been diffuſed more ſlowly. This muſt have been the caſe much more with the art of preparing ſteel, which naturally would be much later than that of preparing iron, and melting tin and copper. The method of preparing ſteel may have been generally known in one country before it was communicated to another, where copper, hardened in the above manner, may have been uſed in its ſtead. The Japaneſe ſtill uſe mirrors of white copper, which conſiſts of a mixture of that metal and tin. The metal of the Chineſe gongs conſiſts of copper, mixed with 18 parts in a hundred of tin, and probably a little nickel; and theſe inſtruments are at the ſame time ſubjected to ſtrong hammering when cold. The Chineſe form their ill-ſhaped razors of iron (not hammered) filed to an edge, and which cut ſo badly, that, after every ſtroke on the beard, they muſt be drawn over a file. Many tribes who have been diſcovered in modern times, and who are unacquainted with the uſe of metals, employ hard kinds of ſtones for making knives, hammers, arrow-heads, axes, &c. Induſtry always ſupplies itſelf with its neceſſaries, and employs for that purpoſe ſuch materials as can be obtained, without taking into account the degree of labour which the preparation of them may require.

XII. *Experi-*

XII. *Experiments on the Nature and Properties of the Pietra fongaja, Lapis fungifer*. By* P. A. GADD †.

STONES in general are in Italy called *Pietra*; a word which ſometimes is uſed to denote the harder ſort of ſtones, as well as ſtones of a certain genus; for example, *Pietra bigia*, *obſidiana*, *nephritica*, &c.; and this is the caſe with the ſo called *Pietra fongaja*. I. I. Ferber may be conſidered as the firſt perſon who gave a deſcription of it in his letters from Italy. He has remarked alſo that a kind of it is in common uſe in the houſes of Naples and Rome; and that he ſaw another kind in the poſſeſſion of M. Fabbroni, at Florence: the firſt kind, which was found in the chalk-hills near Naples, conſiſted of white calcareous ſtalactites, and a number of ſmall roots of vegetables; the latter was a hardened turf, dug up in the neighbourhood of ſome volcanic mountains.

A few years ago, M. Charles Spärre, chancellor of the academy, having been ſo kind as to tranſmit to me a piece of *pietra fongaja*, which he had brought with him from Italy. I analyſed it, and found the reſult as follows:—It burns in an open fire, and emits the ſmell of putrid vegetables. When burnt in a ſtrong fire, the greater part of it becomes dark-grey aſhes. A hundred parts of this hardened turf, loſt about fifteen parts in weight. When fuſed in a ſtronger heat, it is converted into a black opaque flag, which, however, is difficult to be fuſed. When a ſmall part only of this earth is fuſed with borax, the glaſs acquires a dark-green colour. If a little water be poured over the calcined earth, the water exhibits traces of diſſolved pot-aſh, but the earth does not appear to have thereby ſenſibly decreaſed in weight. One hundred parts of the earth, previouſly calcined in a crucible, being analyſed, were found to contain about 45 or 46 of ſiliceous earth, 23 argil, 7 calcareous earth, 20 calx of iron, together with traces of magneſian earth and pot-aſh.

When the *pietra fongaja* is kept in a cellar, and moiſtened

* Muſhroom-ſtone, or muſhroom-bearing ſtone.

† From *Tranſac. Royal Acad. Stockholm for* 1797.

with

with water, it produces a great many eatable muſhrooms, which in Italy are ſerved up at the tables of the great as delicacies. It needs excite no wonder that muſhrooms ſhould grow on the *pietra fongaja*, ſince a multitude of fruitful muſhroom ſeeds are intermixed in this ſoft ſtalactites, as well as with the hardened turf found near volcanoes.

For the information of thoſe who may be deſirous of making muſhrooms continually grow up from the *pietra fongaja*, and of increaſing the quantity, it may be neceſſary to remark, that this effect will be produced, if, according to the experiment of M. Gleditſch, the muſhroom-ſtones kept in cellars be moiſtened with water in which muſhrooms have been waſhed *.

XIII. *On the Volcanic Iſland thrown up in the Neighbourhood of Iceland. By Captain* Von Löwenörn, *of the Daniſh Navy* †.

In the ſpring of the year 1783, a volcanic iſland, thrown up in the neighbourhood of Iceland, excited no little attention. According to the account of the ſea-captain, who firſt ſaw it, exactly at the time when it firſt aroſe, ſmoke and flame ſeemed to riſe from the ſea, but no land or iſland was to be ſeen. It needs excite no wonder, therefore, that the obſerver was thrown into the greateſt conſternation, as he ſays himſelf, when he beheld the ſea on fire! He and the whole crew therefore concluded that the end of the world had arrived, and they all began to prepare themſelves for the awful moment: but, as they heard no trumpet, and as the ſun ſhone in the firmament with his uſual brightneſs, after conſidering what the phenomenon might be, they at laſt concluded that Iceland had been ſwallowed up by an earthquake; that this was a remainder of it; and that the flames aroſe from Hecla, the well-known volcano of that iſland. Full of this idea, they were juſt on the point of re-

* Does not this furniſh a hint to thoſe who rear muſhrooms in gardens on beds of horſe-dung? Edit.

† From *Geographiſche Ephemeriden*, 1799.

turning,

turning, in order to convey intelligence of this event to Denmark; but very luckily they ſoon after diſcovered the coaſt of Iceland.

The place where this volcanic eruption was ſeen, lies only $7^{\frac{1}{2}}$ nautical miles, fifteen to a degree, from the ſouth-weſt extremity of Iceland. Hitherto they had ſeen no land, but recogniſing Iceland, the ſhip reached the place of deſtination, and completed her voyage. Other ſhips, which arrived later, ſaw a ſmall iſland from which the eruption had ariſen; but it always exhibited, as might naturally be expected, a different appearance. The ſame year ſmoke and flames were ſeen to ariſe from the neareſt part of the oppoſite coaſt of Iceland.

As there have been many inſtances of ſuch eruptions from the ſea producing iſlands, this event attracted the notice of government, and the year following orders were given to the ſhips bound to Iceland to examine the new iſland; but it had entirely diſappeared, ſo that no traces of it were to be found. Towards the concluſion of the year, however, an unfortunate accident happened, which, without doubt, was occaſioned by ſunken rocks forming a part of the iſland which had diſappeared.

A Daniſh ſhip of war of 64 guns, called the *Infödſretten*, was expected from the Eaſt Indies, and intelligence had already been received that ſhe had ſailed from the Cape of Good Hope; but after that period no farther account was heard of her till the year 1785, when ſome veſſels returning from Iceland reported, that ſome fragments of this ſhip, together with the long-boat, had been driven aſhore on the coaſt of that iſland. According to every account, and by comparing the different circumſtances, it appears to me certain that the above ſhip was wrecked on theſe rocks, then no longer viſible above the ſurface of the ſea. It is impoſſible that ſuch a large boat could have been conveyed from a ſhip without the hands of men, unleſs the ſhip had been daſhed to pieces. This boat was not only driven to land entire, and in good condition, though without any perſon in it, but there was found in it a box filled with wax candles. At the diſtance of about a quarter of a mile from the boat there were

found various pieces of the ſame ſhip, which could be eaſily known by ſome diſtinguiſhing marks. Theſe parts, of different forms and ſizes, would not have been thrown on ſhore ſo near each other if the misfortune had happened at a greater diſtance; the billows, currents, &c. would certainly have driven them on ſhore at places more remote from each other. Beſides, theſe fragments were driven on ſhore by a wind which blew in a direction from theſe rocks, and nothing elſe of this misfortune had been perceived on the coaſt.

From all theſe circumſtances I conclude that this veſſel had experienced a very bad voyage home from the Cape of Good Hope, for that year eaſterly winds were exceedingly prevalent in the northern ſeas. A great many ſhips, and particularly men of war, preferred going round Great Britain to the paſſage through the Channel. It is probable that the ſhip in queſtion may have been in want of ſome neceſſary, ſuch perhaps as freſh water. The captain, beſides, was well acquainted in Iceland, where I myſelf was with him, ſome years ago, as lieutenant on board a ſhip which he then commanded, and on this account he probably intended to run into ſome of its harbours, but unfortunately ſtruck on the ſunken rocks, the remains of the volcanic iſland. In this diſtreſſed ſituation the crew, no doubt, had recourſe to the only probable means left for ſaving their lives by hoiſting out their long-boat, and while employed in this labour the ſhip, it is likely, went to pieces, and the people were loſt, as none of them were ever ſeen or heard of.

During my expedition to Iceland in the year 1786, I made it my buſineſs to make ſome reſearches in regard to this volcanic iſland, though at that time no ſuſpicion was entertained that the above ſhip had been wrecked in this place; for this conjecture was only a conſequence drawn from my reſearches.

When I arrived in Iceland, where, on account of the buſineſs entruſted to my charge, as well as on account of the obſervations which I was obliged to make for the improvement of charts, I found it neceſſary to remain ſome time with my ſhip in Holmens-hafen, and had at my diſpoſal a ſmall veſſel which was lying there, I ordered Lieutenant,

now

now Captain Grove, to cruife about with this fmall veffel in the neighbourhood of the place where the volcanic iſland had been ſeen. He remained there ſome days, and though he often ſounded with a line of more than a hundred fathoms, found no bottom, ſo that he loſt all hope of making any diſcovery; but, juſt when he was about to return, he obſerved, contrary to all expectation, that the waves broke over ſome rocks lying exactly level with the ſurface of the water. As he now entertained no doubt that he had found what he had been ſent in queſt of, he took the bearings and diſtances from the neareſt part of the coaſt of Iceland, and tranſmitted to me an account of his obſervations.

When the buſineſs of the expedition was ended, and I was about to return at the end of the ſummer, I reſolved to viſit this intereſting point myſelf, and to aſcertain its real poſition by actual obſervation. I took my departure, therefore, from ſome ſmall iſlands, or rocks, which lie before Cape Reikianös, the ſouth-weſt extremity of Iceland, and the outermoſt of which is called the Grenadier's Cap, diſtant $3\frac{1}{2}$ miles ſouth-weſt from the Cape. As the weather was exceedingly favourable, I was ſo fortunate as to obtain its latitude by the meridian altitude of the ſun, and its longitude by a timekeeper. Though the timekeepers which I carried with me were not of the beſt kind, as I had quitted the ſame day one of the ports of Iceland, where I obſerved their rate of going, their relative errors could not be of great importance. I determined, therefore, the poſition of the rock called the Grenadier's Cap at 63° 43′ 40″ north latitude, and 25° 35′ 40″ weſt longitude from the meridian of Paris. This agreed pretty nearly with the obſervations of Verdun de la Crenne, Borda, and Pingré *; eſpecially as I have good reaſon to believe, that, from a want of ſufficient knowledge of the coaſt of this country, they placed Cape Reikianös three minutes too far north, as they make the latitude to be 63° 55′. As I found alſo, by the moſt accurate obſervations that could be made at ſea, that theſe dangerous rocks lie 47° in a direction ſouth-weſt from the true meridian, and juſt four

* *Voyage fait par ordre du Roi en* 1771, 1772.

miles from the before-mentioned Grenadier's Cap, the poſitions of theſe rocks will be 63° 32′ 45″ north latitude, and 20° 2′ 50″ weſt longitude from the meridian of Paris.

As I now proceeded to get a ſight of theſe rocks, Captain Grove, who was on board my ſhip, concluded, from his former obſervations, that we could be at no great diſtance from them, having now quite loſt ſight of the Icelandic coaſt; and the before-mentioned rocks, which lie to the ſouth-weſt from Iceland, though the weather was clear, being now ſcarcely diſcernible. My companion, therefore, aſked whether it was prudent to advance ſo ſtraight upon it. While we were talking on this ſubject the people called out, and immediately every eye was directed to the ſpot, where we ſaw before us the waves breaking over a rock. We immediately put about ſhip, and heaved the lead, which was in readineſs, and found the depth twenty-ſix, and ſoon after forty fathoms, but a little farther no ground was to be found with a line of a hundred fathoms. Some tallow had been put into the bottom of the lead, as uſual, to enable us to determine the nature of the bottom by the ſubſtances which adhered to it. By theſe means we obtained ſmall fragments of ſtone which were entirely lava, or of a volcanic nature. The rock is not large, and the water around it is exceedingly deep. Its height is exactly equal to that of the ſurface of the ſea, or rather a little lower; and for that reaſon it cannot be ſeen till one approaches very near to it, or when the waves break over it.

The origin of the volcanic iſland, which was ſeen in this place in the year 1783, I explain in the following manner:— The rock which now remains formed the crater, which at that period threw up flames and ſmoke. The large quantity of lava which iſſued from it, being accumulated on the bottom of the ſea around the crater, may at length have been raiſed above the ſurface of the ſea, and even to a conſiderable height. But as this volcano lies in a part of the ocean where prodigious billows prevail, and roll over each other throughout a wide extent of ſea, it is probable that ſuch a ſtructure would ſoon be deſtroyed by their violence, eſpecially as there is a great depth of water around it, in which it might eaſily

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be overturned. It is known alſo that the ſame year a conſiderable quantity of pumice-ſtone and volcanic ſubſtances of the like kind, the ſpecific gravity of which was lighter than that of water, was caſt on ſhore in Iceland, and found floating on the ſea by mariners.

Had the eruption taken place in a calmer ſea, and the depth around it been leſs abrupt, the thrown up maſs would have conſolidated itſelf by its own weight, and would have in time become an iſland; of which we have had inſtances in the Archipelago, in the Eaſt Indies, and different parts of the ocean. Had it taken place on the continent, or in an iſland, it would have formed a mountain. It is not neceſſary that a volcano ſhould always ariſe from a mountain: volcanoes have been ſeen to burſt forth in plains; but the invariable conſequence is, that the volcanic matter, by being accumulated, and, as it were, piled up, forms a mountain. Now, as the violence of the waves may have eaſily waſhed away the looſe matter accumulated round the crater, there is no abſurdity in ſuppoſing, that, as the billows rolled over the mouth of the crater, the fire was at length overcome by the water, and the volcano extinguiſhed.

The crater, conſiſting of rock, has remained. It is well aſcertained that a rock exiſted in this place before the eruption; and it is confirmed, by late obſervations, that it exiſts ſtill. An obſcure notion prevailed among the ſeamen who frequented Iceland, that there was a blind rock* in this neighbourhood called *Fugle-Skiär* (Bird's rock). This name I have retained in my charts, though the exiſtence of it is denied by many ſeamen, becauſe they paſſed without ſeeing it. But, under ſuch circumſtances, the teſtimony of one who has ſeen it is of more weight than a hundred who deny its exiſtence becauſe they did not ſee it. This confirms me in the opinion that the crater had exiſted long before in the ſame ſtate.

To conclude, it may not be ſuperfluous to remark, in order to ſtrengthen this opinion, that, nearly in the ſame direction from the ſouth-weſt extremity of Iceland, as already mentioned, there are five ſmall iſlands or rocks, the outer-

* Rocks lying under the water, and which are therefore more dangerous, are by ſeamen called *blind rocks*.

moſt

moſt of which lies at the diſtance of $3\frac{1}{2}$ miles from Cape Reikianös. Between theſe the water is deep; ſhips which go to, or come from, the weſt ſide of Iceland, commonly paſs through them, when they firſt get ſight of the land and rocks. By the Daniſh ſeamen they are called *Fugle-Skiör*, becauſe they are frequented by a great number of ſea-fowl; but by the inhabitants they are called *Eld Eyarne*, (fire iſlands.) May not this afford reaſon to conjecture that in former times they had volcanic eruptions? and the volcano which appeared in the year 1783 may probably have exiſted long before.

XIV. *Experiments on ſome peculiar Matters drawn from Animal Subſtances treated with the Nitric Acid.* By C. Welter *.

THE author having treated ſilk with the nitric acid, to obtain from it the oxalic acid, was ſurpriſed to find that at the end of his proceſs he obtained a ſilky-looking ſalt of a golden-yellow colour, and which, on the approach of a piece of red-hot coal, exhibited all the effects of gunpowder. As he made the experiment only once, he thought it of importance to give a particular account of the proceſs, in order that it might be repeated.

On one part of ſilk he poured ſix parts of nitric acid of the ſhops, adding a little concentrated nitric acid. After it had reſted two days, he diſtilled this mixture. He then poured what had paſſed into the receiver, on what remained in the retort, and filtered the whole. The oxalic acid cryſtalliſing on the filtre, he put the whole again into the retort, and added a pretty large quantity of water, which had ſerved to waſh the filtre. He diſtilled off a part of the water; but as the reſiduum did not cryſtalliſe, returned, by elevating the receiver, what had paſſed over; and, after repeating this operation ſeveral times, obtained for reſiduum an acid liquor of the weight of the ſilk employed, and which contained ſmall granulated cryſtals.

This liquor ſhewed no traces of the oxalic acid. It was yellowiſh, and communicated that colour to the fingers and

* *Bulletin des Sciences*, Vol. II. No. 1.

to

to ſilk. The tint was not in the leaſt weakened by waſhing. C. Welter ſaturated this liquor with lime; and having concentrated it, he added alcohol, which took up a matter of a gummy appearance. The alcohol, diluted with water, being evaporated, there remained a yellow ſubſtance mixed with ſolutions of the nitrat and muriat of lime. Theſe ſalts were decompoſed by carbonat of pot-aſh, and the liquor, ſeparated from the carbonat of lime, was ſubjected to evaporation. It gave golden-coloured cryſtals, which had the fineneſs of ſilk; and detonated like gunpowder, producing a black ſmoke. Theſe cryſtals are ſoluble in water and alcohol, and cryſtalliſe on cooling. They are deprived of their colour by the oxygenated muriatic acid. The ſulphuric acid diſengages from them the odour of the nitric acid. The muriatic acid precipitates, from a ſolution of them, ſmall micaceous, whitiſh, volatile cryſtals, which in the fire exhale a bitter and inflammable ſmoke.

This golden-yellow coloured detonating and cryſtalliſable ſubſtance is by the author called *amer* (bitter); its cryſtals appear to be octaedral. As animal ſubſtances become yellow by the contact of the nitric acid, C. Welter endeavoured to extract *amer* from raw beef; but he found it combined with another ſubſtance, which, like it, could not be altered by the nitric acid. This combination, ſoluble in the concentrated nitric acid, is ſeparated from it by water, under the form of a yellow powder, which does not loſe its colour by expoſure to the air, and which might perhaps be uſeful in painting.

What made C. Welter preſume that this powder is compoſed of *amer* and another ſubſtance, is, that he obtained the latter by treating ſponge with the nitric acid. It is colourleſs, ſoluble in concentrated nitric acid, and ſuffers itſelf to be precipitated by water like the preceding powder.

What has been here ſaid ſeems to ſhew that animal matters treated with the nitric acid give as reſiduum two ſubſtances unalterable by that acid, and which are found either in the ſtate of combination, or ſeparate. It appears that ſilk gives pure *amer*, ſponge gives the ſecond ſubſtance pure, and beef a combination of both. The *amer* is yellow, and ſoluble in water: the combination of both is as inſoluble in water as the ſubſtance obtained from ſponge, but coloured.

XV. *Reflec-*

XV. *Reflections on the Quality of Earthen-Ware, and the Results of the Analysis of some Earths and common Kinds of Earthen-Ware. By* C. VAUQUELIN*.

FOUR things may occasion the difference in the qualities of earthen-ware: 1st, the nature or composition of the matter; 2d, the mode of preparation; 3d, the dimensions given to the vessels; 4th, the baking to which they are subjected. By composition of the matter, the author understands the nature and proportions of the elements of which it is formed. These elements, in the greater part of earthen-ware, either valuable or common, are silex, argil, lime, and sometimes a little oxyd of iron. Hence it is evident that it is not so much by the diversity of the elements that good earthen-ware differs from bad, as by the proportion in which they are united. Silex or quartz makes always two-thirds at least of earthen-ware; argil or pure clay from a fifth to a third; lime from 5 to 20 parts in the hundred; and iron from 0 to 12 or 15 parts in the hundred. Silex gives hardness, infusibility, and unalterability; argil makes the paste pliable, and renders it fit to be kneaded, moulded, and turned at pleasure. It possesses at the same time the property of being partially fused by the heat which unites its parts with those of the silex; but it must not be too abundant, as it would render the earthen-ware too fusible and too brittle to be used over the fire.

Hitherto it has not been proved by experience that lime is necessary in the composition of pottery: and if traces of it are constantly found in that substance, it is because it is always mixed with the other earths, from which the washings and other manipulations have not been able to separate it. When this earth, however, does not exceed five or six parts in a hundred, it appears that it is not hurtful to the quality of the pottery; but if more abundant, it renders it too fusible.

The oxyd of iron, besides the inconvenience of communicating a red or brown colour, according to the degree of

* *Bulletin des Sciences*, Vol. II. No. 2.

 baking,

baking, to the veſſels in which it forms a part, has the property of rendering them fuſible, and even in a greater degree than lime.

As ſome kinds of pottery are deſtined to melt very penetrating ſubſtances, ſuch as ſalts, metallic oxyds, glaſs, &c. they require a fine kind of paſte, which is obtained only by reducing the earths employed to very minute particles. Others deſtined for melting metals, and ſubſtances not very penetrating, and which muſt be able to ſupport, without breaking, a ſudden tranſition from great heat to great cold, require for their fabrication a mixture of calcined argil with raw argil. By theſe means you obtain pottery, the coarſe paſte of which reſembles *breche*, or ſmall-grained pudding-ſtone, and which can endure ſudden changes of temperature.

The baking of pottery is alſo an object of great importance. The heat muſt be capable of expelling humidity, and agglutinating the parts which enter into the compoſition of the paſte, but not ſtrong enough to produce fuſion; which, if too far advanced, gives to pottery a homogeneouſneſs that renders it brittle. The ſame effect takes place in regard to the fine pottery, becauſe the very minute diviſion given to the earths reduces them nearly to the ſame ſtate as if this matter had been fuſed. This is the reaſon why porcelain ſtrongly baked is more or leſs brittle, and cannot eaſily endure alternations of temperature. Hence coarſe porcelain, in the compoſition of which a certain quantity of calcined argil is employed, porcelain retorts, crucibles, tubes, and common pottery, the paſte of which is coarſe, are much leſs brittle than diſhes and ſaucers formed of the ſame ſubſtance, ground with more labour.

The general and reſpective dimenſions of the different parts of veſſels of earthen-ware have alſo conſiderable influence on their capability to ſtand the fire.

In ſome caſes the glazing or covering, eſpecially when too thick, and of a nature different from the body of the pottery, alſo renders them liable to break. Thus, in making ſome kinds of pottery, it is always eſſential, 1ſt, to follow the beſt proportion in the principles; 2d, to give to the particles of

the paſte, by grinding, a minuteneſs ſuited to the purpoſe for which it is intended, and to all the parts the ſame dimenſions as far as poſſible; 3d, to carry the baking to the higheſt degree that the matter can bear without being fuſed; 4th, to apply the glazing in thin layers, the fuſibility of which ought to approach as near as poſſible to that of the matter, in order that it may be more intimately united.

C. Vauquelin, being perſuaded that the quality of good pottery depends chiefly on uſing proper proportions of the earthy matters, thought it might be of importance, to thoſe engaged in this branch of manufacture, to make known the analyſis of different natural clays employed for this purpoſe, and of pottery produced by ſome of them, in order that, when a new earth is diſcovered, it may be known by a ſimple analyſis whether it will be proper for the ſame object, and to what kind of pottery already known it bears the greateſt reſemblance.

	Heſſian Crucibles.	Argil of Dreux.	Porcelain Capſules.	Wedgewood's Pyrometers.
Silex	69	43·5	61	64·2
Argil	21·5	33·2	28	25
Lime	1.	3·5	6	6
Oxyd of iron	8	1	0·5	0·2
Water	18			6·2

Raw kaolin 100 parts.—Silex 74, argil 16·5, lime 2, water 7. A hundred parts of this earth gave eight of alum, after being treated with the ſulphuric acid.

Waſhed kaolin 100 parts.—Silex 55, argil 27, lime 2, iron 0·5, water 14. This kaolin, treated with the ſulphuric acid, gave about 45 or 50 per cent. of alum.

Petuntzé.—Silex 74, argil 14·5, lime 5·5, loſs 6. A hundred parts of this ſubſtance, treated with the ſulphuric acid, gave ſeven or eight parts of alum. But this quantity does not equal the loſs ſuſtained.

Porcelain of retorts.—Silex 64, argil 28·8, lime 4·55, iron 0·50, loſs 2·77. Treated with the ſulphuric acid, this porcelain gave no alum.

XVI. *Eleventh*

XVI. *Eleventh Communication from Dr.* THORNTON, *Phyſician to the Mary-le-bone General Diſpenſary*, &c. &c. &c. *relative to Pneumatic Medicine.*

A REMARKABLE CURE OF AN ULCER OF THE LEG.

MR. RODERICK M'KENNON, aged 67, went in the year 1758 as Aſſiſtant Apothecary to St. George's Hoſpital, where he had his waſhing, board and lodging found him, with a ſuitable ſalary. In June 1795, whilſt in this employ, he went to ſee Dr. M'Nab, who then reſided in Great Suffolk-ſtreet; and as he was at the door, a bitch in the houſe, who had puppies, furiouſly flew at him, and ſeized him near the calf of the leg, making a deep lacerated wound. The wound ſoon after became dreadfully inflamed, poultices were applied, and it was near a fortnight before he made his caſe known to the ſurgeons of the hoſpital. He was now confined to his room, and theſe moſt experienced and eminent practitioners continued their humane attentions to him above a twelve-month, trying a variety of different applications, until, finding his caſe baffle all their endeavours, he was diſmiſſed his employ, and left the hoſpital as incurable. Added to this dreadful and unforeſeen affliction, he had an aſthma, which had exiſted on him above ten years, and was obliged frequently to ſit up the greateſt part of the night with the windows wide open to procure breath. He was now in the vale of years, and with a gloomy proſpect before him; for no ſalary was allowed this almoſt ſuperannuated ſervant of a public charity, to which he had been attached above thirty years; and he had a wife and daughter to provide for. After quitting the hoſpital, Mr. Carpue, a ſurgeon no leſs diſtinguiſhed for zeal than abilities, for ſome months attended him; but finding all his endeavours ineffectual, he reluctantly took his leave of him as incurable. Such was the deplorable ſtate of this unfortunate ſufferer, when Mr. Carpue recommended him for the trial of the oxygen air, uſing theſe very expreſſions: "Poor Roderick has been under Mr. Home's care," (an eminent ſurgeon, brother-in-law to John Hunter,) "in

St. George's Hofpital, which he left as incurable, and fince under my care for feveral months; and fo bad is his cafe, that I am fure if you can cure him you can cure the devil." Being no furgeon, I could have no wifh to accept of fuch a cafe but for the caufe of humanity and the fake of fcience; and I feel extreme delight in faying, that poor Roderick is now perfectly cured, the ulcer is healed, his afthma gone, and, in order that the philofophic world may fee fuller particulars refpecting this extraordinary cure, I am happy to be able to add the following teftimonies:

A Letter to Dr. Thornton, from Mr. Carpue, Surgeon at the York-Hofpital.

DEAR SIR,

I have feen Mr. M'Kennon, and have examined his leg, and think the cure you have wrought on it is indeed very aftonifhing. When he firft came under my care, he laboured under an immenfe ulceration, extending from the external ancle of the right leg, which reached as high as the junction of the tendons of the gaftrocnemii and folæus mufcles. At this period the tendons of the peronæal mufcles had fluffed, and in confequence I applied charcoal, which produced very confiderable good; but upon mentioning this to a friend, who knew the cafe well *, he faid, "It was immaterial what remedy I ufed, for it was a cafe in which he was certain nothing would prove effectual." After this I applied the diluted nitrous acid, and feemingly with advantage; but being obliged to go into the country, I left him under the care of another furgeon; and when I faw him, after an abfence of fix weeks, I found the fore in a very unfavourable condition; and for four months I ufed different applications, but to no purpofe, and I conceived the cafe now to be perfectly incurable, and as fuch mentioned it to you when I had the pleafure of meeting you at Mr. Heavifide's: and I then propofed him to you for the trial of the oxygen air, as his cafe, if fuccefsful, would prove moft decidedly its efficacy; for, in the multitude of fore legs I have at-

* We believe this to be Dr. Bailey, phyfician to St. George's Hofpital.

tended,

fended, I muſt acknowledge I never ſaw a worſe caſe, old Weſt-India ſores excepted.

I have the honour to be, Sir,
Your obedient ſervant,
J. C. CARPUE.

Having referred Mr. M'Kennon to Mr. Spencer, a ſurgeon in Charlotte-ſtreet, Fitzroy-ſquare, who adminiſters the pneumatic remedies, I received the following letter:

A Letter to Dr. Thornton from Mr. Spencer.

SIR,

I here encloſe the treatment and the progreſſive cure of the ulcer which occupied the external ancle of the right leg of Mr. M'Kennon. For ſix weeks, by your direction, he daily took a gallon of oxygen air, mixed with four times that quantity of atmoſpheric air. The ulcer diſcharged properly, but ſeemed to heal very ſlowly: in conſequence I gave him a double doſe, and after a fortnight it produced very feveriſh ſymptoms, when he took by your order ſome purgatives, and then he reſumed his uſual doſe of ſuperoxygenated air daily, until the ulcer, diminiſhing by degrees, was at laſt completely healed, there being no diſcharge, the whole cicatriſing, and the new-formed ſurface looking extremely healthy. During this period no particular application or dreſſings were made uſe of by me, nor any medicine directed by you, but what before he ſaid he had taken gallons of; ſo that I attribute his extraordinary cure entirely to the efficacy of the oxygen air. Happy in being able to give my teſtimony to ſo remarkable a caſe, I have the honour to be, dear Sir,

With the profoundeſt reſpect,
Your obedient humble ſervant,
T. SPENCER.

REMARKS BY DR. THORNTON.

As Mr. M'Kennon took bark, ſome of the Faculty may not be willing to give to the oxygen air the merit in this cure: I will therefore endeavour to ſtate ſhortly my reaſons for attributing every thing to this new remedy.

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1. The operation of bark had been before tried; he had taken, he ſaid, gallons of it.

2. When I firſt ſaw him, the ſore, and muſcles ſurrounding it, were wholly inſenſible; he did not feel a needle piercing them, nor could he perceive even the corroſive operation of cauſtic.

3. After inhaling the vital air but a few days, ſenſibility was reſtored, as both Mr. M'Kennon and Mr. Carpue witneſſed.

4. Having cleanſed the wound, it would remain dry; but even whilſt inhaling the vital air, the whole ſurface was immediately covered with a fine dew, as Dr. Monro and others witneſſed.

5. To ſhew the progreſs of amendment whilſt inhaling the ſuperoxygenated air, I am happy to be able to lay before the philoſophic world the following teſtimony of an impartial obſerver, Dr. Douglas of Baliol-College, Oxford:

"Towards the middle of March 1798, I firſt ſaw Mr. M'Kennon. He had then a large and very foul ulcer, extending ſome inches above the right ancle. From that time to the preſent (April 30) I have repeatedly ſeen him, and *each time could not poſſibly fail to be ſenſible of a moſt manifeſt improvement.* At preſent *the ulcer is diminiſhed at leaſt one-half in ſize ſince I firſt ſaw him*; the edges have a fine healthy appearance, and its general ſurface is aſtoniſhingly altered for the better."

6. When the oxygen air was left off, the ſore remained ſtationary, and viſibly improved when he again reſumed it.

7. My ſtrongeſt argument, however, is the ſucceſs in this and in other caſes equally deſperate.

Mr. Munt had been before cured of a ſore leg of eighteen years ſtanding.

Mr. Atwood was cured of a ſore leg of two years. When I aſked Mr. Cruikſhank whether it was true he had condemned the leg? he anſwered, with his uſual emphaſis, "I not only condemned his leg, but his life; for he was of ſo waſhy a conſtitution that he could not have loſt the one without the other."

Next the cure of Patterſon. When I ſent him to ſhew

his leg to Mr. Cruikſhank, which ſtill poſſeſſes the marks of numerous ulcers, ſeeing varicoſe veins, this experienced ſurgeon ſaid: "Tell Dr. Thornton that he is miſtaken if he ſuppoſes he has made a permanent cure; for varicoſe ulcers were never cured without an operation, which, if he wiſhes, I will perform." The man, frightened at firſt, and then aſtoniſhed, replied: 'Sir, I have been cured perfectly now theſe three years.'—"That alters the caſe," anſwers this diſtinguiſhed anatomiſt; "then tell Dr. Thornton that he has performed a moſt wonderful cure."—Patterſon ſtill continues well; nor does there ſeem the ſmalleſt cauſe to ſuſpect a relapſe.

The cure of Mr. Wilkinſon*, who had a ſore leg twelve years, is not leſs extraordinary. In this caſe I obſerved a peculiar phenomenon, alone explicable by the operation of the oxygen air. The fingers of both hands at their ends looked very red, as red as raw meat, were ſwollen, and felt very painful. The ſame was mentioned to me in private converſation by Dr. Beddoes in a patient of his, who, finding an aſthma relieved by a ſmall doſe of vital air, took as much as he could at one time, produced a fever, and this ſame phenomenon I have juſt mentioned above.

This leads me to repeat an obſervation I have before often expreſſed, that oxygen air promiſes to be an uſeful remedy in ſore legs; for why have we not ſore arms? The nearneſs of this part to the heart ſeems to be the only philoſophic reaſon; and therefore a direct powerful ſtimulus to the heart, as oxygen air, promiſes the moſt certain good, aided by the invigorating effects of bark, ſteel, and other tonic medicines; not but that I would adviſe, where it can be properly done, as in hoſpitals, trials to be made with the vital air without medicine, to prevent all cavil; although it is undoubtedly unimportant to the ſufferer by what means he is treated, ſo that he is *but cured*; and, until the contrary is proved, I ſhall ever think, that medicines, judiciouſly employed, certainly cannot *impede* the operation of oxygen, but may *aſſiſt*.

I now take my leave of the philoſophic world, not from dearth of materials, for I have cures by me yet more im-

* The caſes of Mr. Munt, the Rev. Mr. Atwood, Patterſon, and Mr. Wilkinſon, are related in Dr. Beddoes's Conſiderations on Factitious Air.

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portant than thoſe before related by me, that deſerve, I think, to be recorded; and daily experience, in a large public charity*, where the airs are adminiſtered under my direction, would afford me other frequent opportunities; but according to my promiſe, mentioned in my firſt communication, I was to continue writing on this ſubject only until the eſtabliſhment of the Pneumatic Inſtitution under Dr. Beddoes, which I am happy to announce, and that ſhortly the public may expect a periodical quarterly publication, relating chiefly to this important inveſtigation, from the maſterly pen of that eminent phyſician.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

GERMANY.

THE Electoral Academy of Sciences at Manheim has propoſed the following queſtion as the ſubject of a prize for the year 1801:—

"Are the azotic (*ſlick*) gaſes, which are produced from ſo many totally diſſimilar ſubſtances, and in ways ſo different, exactly the ſame in all their chemical properties and baſes (ſimple azotic gaſes,) with that of the atmoſphere? and has the nitrous acid the ſame azote for its acidifying baſe, as the atmoſpheric azotic gas?

"The partizans of the antiphlogiſtic doctrine ſeem to admit both, but without ſatisfactory proof. 1, Since, for want of ſufficiently accurate examination, they admit in each of theſe gaſes all the known properties of azotic gas, becauſe they deſtroy animal life, extinguiſh flame, and manifeſt no acid properties: but, 2, after all their analytic and

* The Mary-le-bone General Diſpenſary.

ſynthetic

fynthetic proofs, they have ſtill left well-founded doubts, (*a*) whether the electric ſpark, in its paſſage through oxygen or azotic gas, does not itſelf undergo a chemical decompoſition, and furniſh the baſis of the nitrous acid; (*b*) and how, by the ſame means, (a red heat, and the electric ſpark,) the nitrous acid is decompoſed into oxygen and azotic gaſes, and can be again recompoſed from them; and, (*c*), ſince oxygen and azotic gaſes have ſo great an affinity for each other in the atmoſphere, why, when the former is added in a ſufficient quantity, imperfect nitrous acid is not immediately produced, as is the caſe when oxygen gas is added to the azote in nitrous gas, by which perfect nitrous acid is immediately produced?

"The papers on this ſubject, written either in Latin or French, muſt be tranſmitted before the 1ſt of November 1800, to M. I. Kenedy, ſecretary to the academy, with the name of the author, in a ſealed note, and any motto choſen at pleaſure. The prize is a gold medal, of the value of 50 ducats."

HOLLAND.

The Dutch Society of the Sciences at Haerlem propoſed in 1793, and afterwards in 1796, the following queſtion: "What light has Lavoiſier's ſyſtem, and his method of examining organic ſubſtances, furniſhed towards a more accurate knowledge of the human body?" But, as no ſatisfactory anſwer was received, they have propoſed it again in the following manner:—

I. As a great number of new diſcoveries have been made ſince the queſtion was firſt propoſed, and as the circumſtances of it have thereby acquired more extent than to admit of their being properly comprehended in a ſingle treatiſe, the Society have reſolved to divide the different objects of it into three new queſtions for the preſent year, and to fix the period of receiving anſwers at the 1ſt of November 1800.

1. What light has the new chemiſtry thrown on the phyſiology of the human body?

2. How far has the light, thrown on the phyſiology of the human body, contributed to a better knowledge than before of the nature and cauſes of certain diſeaſes; and what uſeful

conſequences, more or leſs confirmed by experience, can be deduced from it in regard to the practice of medicine?

3. How far has the new chemiſtry contributed to afford an accurate idea of the mode of action of different internal and external medicines, which have been long uſed, or only lately recommended? And, what advantages can ariſe from a more accurate knowledge of this point in regard to the treatment of certain diſeaſes?

As ſome learned men have introduced hypotheſes built on too weak a foundation, in regard to the application of the principles of the new chemiſtry to phyſiology, pathology, and therapeutics; and as this is highly prejudicial to the progreſs of theſe ſciences, to which the new chemiſtry, however, promiſes ſo much light, if, according to Lavoiſier's rule, we admit nothing in chemiſtry, or the employment of chemical principles, but what is founded on deciſive experiments, the Society requires, that thoſe who are inclined to anſwer theſe queſtions will make an accurate diſtinction between what is proved, and what is merely hypothetical; and that, in regard to hypotheſes, the candidates will confine themſelves to a bare mention of them, and of the few grounds on which they reſt; becauſe the principal point which the Society wiſhes to obtain is, that thoſe who follow the medical or chirurgical profeſſion in Holland, and who are not yet ſufficiently acquainted with the progreſs of the new chemiſtry, and its application, on well-founded principles, to phyſiology, pathology and therapeutics, will procure ſuch works as may be beſt calculated to inform them what light the new chemiſtry has actually thrown on theſe ſciences; and what facts are founded on too weak grounds; and what have been too raſhly adopted, or are ſtill too doubtful to be depended on. Each of theſe papers will be examined ſeparately: thoſe, therefore, who wiſh to anſwer more than one queſtion, muſt ſend a paper for each.

II. The Society requires a plan, capable of being carried into execution, for rendering productive the large uncultivated diſtricts of the republic, particularly in Guelderland, Overyſſel, Drenthe, and Dutch Brabant.

III. The

III. The following prize queſtions are again propoſed:—

1. A natural hiſtory of the whale; in order to furniſh hints for its being more eaſily diſcovered and caught, and afterwards converted to uſe. This queſtion to be anſwered before the 1ſt of November 1802.

2. What has experience taught in regard to the uſe of certain animals which in the Netherlands appear to be hurtful; and what means are to be employed for extirpating them? For this queſtion no period is defined.

3. What indigenous plants, the virtues of which have been hitherto unknown, might be employed in the apothecaries ſhops in Holland to ſupply the place of foreign medicines? The virtues of them muſt be eſtabliſhed, not by foreign teſtimony, but by the teſtimony of natives of the country.—The time for anſwering this queſtion is indefinite.

4. What indigenous plants, not yet employed, might be introduced into uſe as good and cheap food? And, what foreign nutritive plants might be cultivated for the ſame purpoſe?—No definite time.

FOSSIL WOOD FOUND AT A GREAT HEIGHT.

In a paper lately read before the French National Inſtitute, it appears that C. Villars, Profeſſor of Natural Hiſtory, of Grenoble, ſaw, near a glacier in the department of Iſere, ſome foſſil wood buried among turf at the height of 2320 metres above the level of the ſea, and 850 metres above the moſt elevated line at which wood grows at preſent. The mountain on which this intereſting diſcovery was made, is that of Lans, in the canton of Oiſans. The trees found there are, mountain-aſh, birch, and the common larch. The roots and part of the trunks can be plainly diſtinguiſhed. The laſt of theſe trees does not grow at preſent in the neighbourhood.—The author of this memoir aſcribes the greater degree of cold, which now prevails on theſe mountains, to two principal cauſes: firſt, the valleys becoming deeper, which has changed the elevation of the ſummits in regard to their baſes and the ſurrounding countries: the ſecond is the deſtruction of the ancient foreſts, which had gradually extended themſelves to great heights, but which, when once deſtroyed, cannot grow

up again at the ſame heights, becauſe the trees are deprived of that mutual ſhelter which they afforded to each other.

NEW THEORY OF RESPIRATION.

Profeſſor Herholdt read lately, before the Academy of Sciences at Copenhagen, a memoir reſpecting ſome experiments made by him and M. Rafn on living animals, in order to diſcover the mechaniſm of reſpiration; having in view, at the ſame time, the cure of wounds in the breaſt. The profeſſor ſhewed that the beſt authors on ſurgery have hitherto explained the mechaniſm of reſpiration in a manner diametrically oppoſite to what it really is; ſo that, by applying their theory to the cure of wounds in the breaſt, they have followed a method altogether falſe. According to his experiments the lungs have not, as has been maintained, an expanſive force peculiar to them, but the movement is performed by the action of the diaphragm, to which ſufficient attention has not hitherto been paid. When there are wounds in the breaſt, the atmoſpheric air enters by them on inſpiring into the cavities of the thorax, and iſſues on expiring. This has been proved by experiments made on horſes, dogs and cats.

M. Herholdt and Rafn, in examining the manner in which the frog breathes, remarked, that this animal is without a diaphragm, and that its lungs at the ſame time have no expanſive force; but that a ſmall membrane, by means of which it can ſhut its mouth hermetically, diſcharges the function of the diaphragm; ſo that, when it is prevented from ſhutting its mouth by inſerting into it a ſmall rod, the animal dies in a few minutes, becauſe it is no longer able to breathe. When it is ſuffered to ſhut its mouth before it is entirely dead, or when it is only in a ſtate of aſphyxia, it ſoon recovers. If a frog be deprived of this membrane, by cutting it entirely off, or only in part, ſo that its mouth can no longer be hermetically ſhut, it expires in a longer or ſhorter time according to the ſize of the aperture made: on the firſt view it appears very paradoxical that man, as well as the greater part of animals, loſes his life by not being able to breathe when his mouth and noſe are ſhut, and that the frog dies becauſe it cannot breathe when its mouth is opened. The explanation

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of this phenomenon, however, is eaſy when we recollect that the lungs have no expanſive force. In conſequence of this new theory, M. Herholdt has ſucceeded in curing very dangerous wounds made in the breaſts of dogs.

The above experiments, which were communicated to the Philomatic Society at Paris, by M. Manthey, were repeated with ſucceſs, by the commiſſioners of that Society, on frogs and ſalamanders. If a gag be put into the mouth of one of theſe animals, ſo as to prevent it from ſhutting its mouth, it dies at the end of half an hour. The reſpiration is performed as follows:—The mouth being abſolutely ſhut, the frog dilates its throat, and the air ruſhes in by the noſtrils; it afterwards contracts its throat, and the air penetrates to the lungs, becauſe, no doubt, there is a ſmall valve in the noſtrils which prevents it from eſcaping by the ſame way that it entered; for the membrane, which the Daniſh authors aſſert they obſerved in the mouth, could not be ſeen by the commiſſioners. Lizards and ſerpents, which have ribs, breathe like other animals; and a forced opening of the mouth does not kill them.

MEDICAL PNEUMATIC INSTITUTION.

All who wiſh well to the intereſts of humanity will rejoice to learn that this inſtitution, as uſeful as novel, is at laſt ſo far eſtabliſhed that there is every reaſonable ground to believe the objects of it will now receive that ample and fair inveſtigation their importance demands. The learned, ingenious, and meritoriouſly perſevering founder of it, has already publiſhed a *Notice of Obſervations made at the Inſtitution*, which contains ſome highly intereſting remarks on the effects of a gas not before applied to medicine, but which promiſes, in ſkilful hands, to be one of the ſafeſt and moſt powerful agents hitherto diſcovered.

Mr. Davy, ſuperintendant of the inſtitution, than whom it would hardly be poſſible to find one better qualified for the ſituation, having made ſome experiments on *dephlogiſticated nitrous gas*, ſo named by its diſcoverer Dr. Prieſtley, which proved that its compoſition, properties, and mode of action, had been miſtaken by the lateſt experimenters, was induced

to inhale it. Dr. Beddoes relates the circumſtances in the following words:

"The firſt inſpirations of the gas produced giddineſs, fulneſs of the head, and, in ſhort, feelings reſembling thoſe of incipient intoxication, but unaccompanied by pleaſurable ſenſation. At this next experiment I was preſent. The quantity was larger, and the gas more pure. The ſcene exhibited was the moſt extraordinary I had ever witneſſed, except in the caſe of that epileptic patient, whom I have deſcribed (*Conſiderations on Airs*, part iv. p. 13.) as agitated, in conſequence of the reſpiration of oxygen gas, with a long ſucceſſion of the moſt violent movements. The two ſpectacles differed, indeed, eſſentially in one reſpect. In the former every thing was alarming: in the latter, after the firſt moments of ſurprize, it was impoſſible not to recognize the expreſſions of the moſt ecſtatic pleaſure. I find it entirely out of my power to paint the appearances, ſuch as they exhibited themſelves to me. I ſaw and heard ſhouting, leaping, running, and other geſtures, which may be ſuppoſed to be exhibited by a perſon who gives full looſe to feelings excited by a piece of joyful and unlooked-for news. As in the caſe of the epileptic patient, *no wearineſs or depreſſion followed*; ſo, in this caſe, *no exhauſtion or languor or uneaſy feeling took place*. The experiment Mr. Davy has very frequently repeated, and generally with the higheſt pleaſurable ſenſations; and, except under particular circumſtances, with conſiderable muſcular exertions, which have not in any inſtance been ſucceeded by fatigue or ſadneſs."

A number of perſons afterwards inhaled the ſame gas: the following extracts will convey ſome idea of the very ſingular effects produced by it:

"Mr. J. W. Tobin (after the firſt imperfect trials), when the air was pure, experienced ſometimes ſublime emotions, with tranquil geſtures; ſometimes violent muſcular action, with ſenſations indeſcribably exquiſite; no ſubſequent debility—no exhauſtion.—His trials have been very numerous. Of late he has felt only ſedate pleaſure. In Mr. Davy the effect is not diminiſhed,

Mrs.

" Mrs. Beddoes—Pretty uniform pleaſurable ſenſations—propenſity to muſcular exertion, could walk much better up Clifton Hill—has frequently ſeemed to be aſcending like a balloon, a feeling which Mr. Burnet ſtrongly expreſſed.

" Mr. James Thomſon. Involuntary laughter—thrilling in his toes and fingers—exquiſite ſenſations of pleaſure—a pain in the back and knees, occaſioned by fatigue the day before, recurred a few minutes afterwards. A ſimilar obſervation we think we have made on others; and we impute it to the undoubted power of the gas to increaſe the ſenſibility, or nervous power, beyond any other agent, and probably in a peculiar manner.

" Mr. Stephen Hammick, Surgeon of the Royal Hoſpital, Plymouth. In a ſmall doſe, yawning and languor.—It ſhould be obſerved, that the firſt ſenſation has often been diſagreeable, as giddineſs; and a few perſons, previouſly apprehenſive, have left off inhaling as ſoon as they felt this.—Two larger doſes produced a glow, unreſtrainable tendency to muſcular action, high ſpirits, and more vivid ideas.

" Mr. Robert Southey, for many hours after the experiment, imagined that his taſte and ſmell were more acute, and is certain that he felt unuſually ſtrong and cheerful. In a ſecond experiment, he felt pleaſure ſtill ſuperior—and has ſince poetically remarked, that he ſuppoſes the atmoſphere of the higheſt of all poſſible heavens to be compoſed of this gas.

" Robert Kingleake, M.D. Additional freedom and power of reſpiration, ſucceeded by an almoſt delirious but highly pleaſurable ſenſation in the head, which became univerſal, with increaſed tone of the muſcles. At laſt an intoxicating placidity abſorbed for five minutes all voluntary power, and left a cheerfulneſs and alacrity for ſeveral hours. A ſecond ſtronger doſe produced a perfect *trance* for about a minute; then a glow pervaded the ſyſtem. The permanent effects were, an invigorated feeling of vital power and improved ſpirits. By both trials, particularly by the former, old rheumatic feelings ſeemed to be revived for the moment.

" Mr. Wedgwood, after breathing ſome time, threw the bag from him, kept breathing on laboriouſly with an open mouth, holding

holding his noſe with his left hand* without power to take it away, though aware of the ludicrouſneſs of his ſituation—all his muſcles ſeemed to be thrown into vibratory motion—he had a violent inclination to make antic geſtures—ſeemed lighter than the atmoſphere, and as if about to mount. Before the experiment, he was a good deal fatigued after a very long ride, of which he permanently loſt all ſenſe. In a ſecond experiment nearly the ſame effects, but with leſs pleaſure. In a third, much greater pleaſure."

The caſes in which its effects were prejudicial are alſo ſtated with great candour. Theſe were of the hyſterical kind; but the conſideration of the whole of theſe phenomena led to a happy application of the gas to the cure of palſy, and of diſeaſes proceeding from a defect of nervous energy; and ſeveral inſtances of ſucceſs are ſtated, which are well deſerving of attention.

"We deſire further," ſays the author, "to try the effects of our new agent in palſy, and in the various caſes of *true* deficiency of nervous power, which we have well learned to diſtinguiſh from caſes of relative defect of irritable power. As the limits of the efficacy of every remedy ought to be determined, we ſhall not ſhrink from any caſe, by reaſon of its inveteracy. We intend to oppoſe our Nepenthe to the equable decay induced by time and intemperance; and we hope to palliate ſome of the evils of extreme old age itſelf.

"We are emboldened by experience to pledge ourſelves for the ſafe employment of the gas. We ſhall, indeed, be ſadly diſappointed if it do not ſometimes prove the moſt delicious of luxuries, as well as the moſt ſalutary of remedies. In ſaying this, it may be allowed me to ſuggeſt to thoſe, who have not attended to the tenor of my opinions, that I now for the firſt time venture to hold forth theſe hopes. However urgently I may have recommended the inveſtigation, my language, with regard to its iſſue, has always been, that *I would not anſwer for the diſcovery of a gaſeous remedy in any denomination whatever of diſeaſe. That natural or forced decay may be repaired, and the faculty of pleaſurable ſenſation renovated,*

* This was practiſed in all the experiments.

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is now no longer a mere conjecture ſupported by looſe analogies—we ſee the ſtrongeſt probabilities daily accumulating in favour of the opinion. It muſt only be remembered, that ſo deſirable a change cannot be effected by the agent applied in *any* manner to *any* conſtitution. It muſt be *properly* uſed in *proper* caſes.

"Conſidering the preſent abundance of expert chemiſts, we cannot preſume that others will not be able to prepare the gas perfectly without our inſtructions. Nevertheleſs, thoſe who attempt to uſe it medicinally ſhould be apprized that the utmoſt care is neceſſary in its preparation and employment. A deleterious, inſtead of a ſalutary fluid, as the author can atteſt from his own painful experience, may eaſily be obtained. Probably neither Dr. Prieſtley, nor the Dutch chemiſts, ever procured that which can be reſpired with ſafety. The difference, and its cauſes, will hereafter be pointed out.

"If, therefore, in conſequence of that promptitude which journals afford, experiments tending to invalidate any part of the preceding ſtatement ſhould be publiſhed, we hope that its perfect fidelity will not on that account be doubted. The experimenter will be found to have employed a different agent from ours. This we have no heſitation in declaring that we ſhall be able to ſhew."

From the preceding ſhort extracts, our readers will ſee what an intereſting field is opened for the advancement of phyſiology and medicine; and we hope that ſome will be induced to aſſiſt an inſtitution, which ought not to be confounded with ordinary charitable foundations. It is eſtabliſhed for the ſake of relieving from a ſpecies of diſtreſs, from which no degree of affluence can exempt—namely, that which ariſes from the imperfect ſtate of medicine. Subſcriptions are received by Mr. Couts, Banker, Strand, London; and by Mr. Savery, Banker, Briſtol.

CHROMAT OF IRON.

C. Pontier, correſpondent to the *Journal des Mines*, tranſmitted lately to the Cabinet of the Houſe of Inſtruction, among other intereſting minerals, a ſubſtance in an irregular maſs, of a dark brown colour, having a metallic ſplendour

and mean hardneſs, the ſpecific gravity of which was found to be 4·0326. C. Pontier found it in the department of Var, à la Baſtide de la Carrade, near Gaſſin, and conſidered it as the brown blende, to which indeed it has a conſiderable degree of reſemblance, except that its ſpecific gravity is far greater. This ſubſtance being analyſed in the laboratory of the mines by C. Taſſaert, was found to be chromat of iron, that is to ſay, a metallic ſalt formed by the combination of iron with the acid ariſing from the new metal diſcovered by C. Vauquelin, to which he gave the name of chrome. It ſeemed to contain, in 100 parts, 63·6 of that acid, 36 of iron; loſs, 1·4. Chemiſts may now flatter themſelves that chrome, which hitherto has been found only in the red lead of Siberia, in the ruby and the emerald, may be obtained in ſufficient abundance to enable them to ſubject it to new reſearches.

C. Vauquelin and Taſſaert, by continuing their experiments on this ſubſtance, have aſcertained the following facts:

1. It does not melt alone by the blow-pipe, but with borax, to which it communicates a green colour like that of the emerald.

2. It is ſoluble in the muriatic acid, but ſlowly, and in ſmall quantity. From its ſolution, which is of a greeniſh blue colour, it is precipitated white by alkalies.

3. It is ſoluble in oxygenated muriatic acid. From this ſolution, almoſt colourleſs, it is precipitated of a reddiſh brown by alkalies, and of an orange yellow by nitrat of lead.

4. It is not decompoſable by potaſh, or the carbonat of potaſh, without the aſſiſtance of a degree of heat which carries it to incandeſcence.

5. It may eaſily be reduced by the known means. It then gives an alloyed maſs, having only an external metallic aſpect, eaſily fuſed with borax; brittle, but hard as ſteel.

6. This maſs is exceedingly difficult to be decompoſed. It muſt be treated ſucceſſively and ſeveral times with potaſh, which combines with the chromic acid and diſſolves it, and with the muriatic acid, which diſſolves the oxyd of iron.

7. Other

7. Other experiments have proved the preſence in this mineral of ſilex and alumine; ſo that C. Vauquelin and Taffaert believe it to be compoſed of the following ſubſtances:

Chromic acid - -	43
Oxyd of iron - -	34·7
Argil - - -	20·3
Silex - - -	2

They are of opinion alſo, that, the chromic acid being in ſufficient quantity to ſaturate the oxyd of iron, this mineral is a triple combination of the chromic acid, the oxyd of iron, and argil.

8. The oxyds of chrome, or the chromic acid, may be employed in the porcelain manufacture. When pure, they give an emerald green, more beautiful than that of copper; and, mixed with lead or antimony, a canary-bird green. They may be employed alſo in painting, by ſeparating the acid from the iron, and combining it afterwards with different metallic oxyds by double affinities.

METHOD OF CHECKING DECAY IN TREES.

The cheſtnut lives a long time, and often attains to an extraordinary ſize; but unfortunately the texture of its wood alters under certain circumſtances: it becomes ſoft, falls into duſt, a cavity is gradually formed in the heart of the tree, and this cavity, by the progreſs of the decompoſition, becomes ſtill larger; ſo that, at laſt, the trunk ſeems to conſiſt of nothing but bark; and being too weak to ſupport the branches, and reſiſt the violence of hurricanes, it cannot long exiſt. It is by ſimilar changes and decompoſitions of the ligneous principle, that trees, which have been growing for ages, are ſeen to periſh in a very little time.

C. Chaptal, in travelling through different parts of France, and particularly the Cevennes and the department of Allier, obſerved, that a great number of cheſtnut-trees were hollow, and charred over the whole internal ſurface. He was told by the inhabitants of the country, that this proceſs was employed to ſtop the progreſs of the caries, which otherwiſe would deſtroy the whole tree. When they obſerve that this

diſeaſe, which is very common and exceedingly fatal to the cheſtnut-tree, begins to make any progreſs, and to excavate the trunk, they collect heath and other vegetables, and ſet fire to them in the cavity, till the whole ſurface is completely charred. It happens very rarely that the tree periſhes by this operation, and it is always found that this remedy ſuſpends the effect of the caries. It is practiſed with the ſame ſucceſs on the white oak. By comparing the effects of the cautery on the human body, in analogous caſes of degeneration, we perceive a new ſimilarity between diſeaſes which affect the living organiſed beings of the two kingdoms, and between the remedies by which they may be checked.

ON THE GRAVITY OF TUNGSTEN.

The difficulty of bringing tungſten to complete fuſion has hitherto prevented the ſpecific gravity of this new metal from being with certainty determined. Some fixed it at 17·6, according to the experiments of the brothers d'Elhuyar; but many could not believe it to be ſo conſiderable. C. Guyton lately obtained a well-formed button, of the weight of 35 grammes, in a three-blaſt-furnace, where the intenſity of the fire may be carried to about 185° of Wedgewood: but this button having broken by the preſſure of the vice, into which it was put in order to be ſawn, there was diſcovered at its centre a part not agglutinated, which, by expoſure to the air, ſpeedily aſſumed a purple colour, ſimilar to that which the beſt fuſed tungſten, of ſuch a degree of hardneſs as to render a file brilliant, exhibits on its ſurface. It reſults, from theſe experiments, that the ſpecific gravity of the fuſed portion, ſeparated from that which was only fuſed imperfectly, was 8·3406; that the infuſibility and brittleneſs of this metal leave no other hopes of applying it to the arts, though there are abundant mines of it in France, but by mixing it with other metals, or by the property diſcovered in its oxyds of yielding fixed colours, and fixing vegetable colours.

FLUAT OF ARGIL.

C. Vanquelin has received from Denmark a white lamellated mineral brought from Greenland, which proves to be

fluat of argil, an earthy neutral ſalt never before found in a natural ſtate.

ASTRONOMY.

C. Lalande has lately preſented to the National Inſtitute an account of his obſervation of the laſt oppoſition of Mars, with the calculations on that ſubject; and by comparing it with that of 1790, he has found that only 58 ſeconds are to be deducted from the place of the aphelion of Mars, employed in the laſt edition of his Aſtronomy. He announces a large work on this planet by C. Lefrançais-Lalande, his nephew.

C. Lalande has alſo given the calculation of eclipſes of the ſun or ſtars obſerved for ſome years, to deduce from them the poſition of different cities. He has found Hamburgh to be 30′ 9″ from Paris; Cobourg 34′ 30″; Mulheim 21′ 20″; Halle 38′ 28″; and Konigſberg 1ʰ 12′ 35″.

MONUMENT TO LINNEUS.

A monument has lately been erected in the cathedral of Upſal, to the memory of the great Linneus. It conſiſts entirely of the porphyry of Elfwedal; is properly a pedeſtal in the form of an altar, the ſteps of which are of the brown ſtone of Oeland; and ſupports a medallion containing a buſt of Linneus. The following is the inſcription:

CAROLO A LINNE,
BOTANICORUM PRINCIPI,
AMICI ET DISCIPULI.
M,DCC,XCVIIII.

SUBSTITUTES FOR COFFEE.

Count von Burgſdorf has lately laid before the Royal Academy of Sciences at Berlin, ſamples of coffee prepared from the ſolid parts of the beet-root, after the ſaccharine juice has been expreſſed.

After the moſt careful examination of all the ſubſtitutes for coffee hitherto employed, the Academy of Sciences at Petersburgh has made known that the acorn is the beſt, as it poſſeſſes, when proper means are uſed to communicate to it the oily properties, all the requiſite qualities of coffee. To communicate theſe oily properties, the following proceſs is recommended:

commended: When the acorns have been toaſted brown, add freſh butter, in ſmall pieces, to them while hot in the ladle, and ſtir them with care, or cover the ladle, and ſhake it in order that the whole may be well mixed. By theſe means you will obtain the beſt and moſt harmleſs ſubſtitute for coffee hitherto employed.

SUBSTITUTE FOR TINCTURE OF BARK.

Dr. G. F. C. Fuchs, teacher extraordinary of medicine at Jena, has prepared from the ripe fruit of the horſe cheſtnut, *Aeſculus hippocaſtanum* LINN. when diveſted of the huſks, an extract, which, according to his experiments, may be uſed, perhaps, inſtead of the expenſive *extractum chinæ*, ſince the bark of this tree has been long known as a ſubſtitute for cinchona.

SUBSTITUTE FOR GALLS.

A German apothecary, named Trömer, has lately diſcovered, that the excreſcences or knots on the roots of young oaks may be uſed as a ſubſtitute for galls. Theſe excreſcences are produced, in the ſame manner as the galls, by an inſect, which, after pricking them, depoſits its eggs in them. With vitriol of iron, in the ſame proportion as galls, they give a beautiful black ink, and may be uſed alſo in dyeing. In the ſpring, theſe excreſcences may be found in great numbers on the ſmall roots of the oaks, particularly on the ſouth ſide, often about a hand's-breadth below the earth. Thoſe found in ſummer have, for the moſt part, ſmall apertures, capable of admitting a moderate-ſized needle, but they are no longer filled with eggs. At this period they are more woody, and not ſo good; and therefore they ought to be collected in the ſpring.

TRAVELS IN AFRICA.

Mr. Horneman, whom we ſome time ago announced to have ſet out with the view of exploring the interior of Africa, has written a letter to Sir Joſeph Banks, from Tripoli. He had travelled from Cairo, in Egypt, through the Lybian Deſert, to Fezzan, the largeſt Oaſis in the Great Sahara; a route hitherto unexplored by any European, whoſe travels have

have been communicated to the public. In the journey from Cairo to Fezzan, he halted at Sewah, which, from the notices of Mr. Brown, ſome months ago, had been clearly aſcertained to be the Oaſis of Ammon.

Mr. Horneman's new obſervations, made at his leiſure on the ſpot, now place this matter beyond all manner of doubt. Mr. H. was too late this ſeaſon for the caravan that goes from Fezzan to Soudan, comprehending under that name Hourſes, Caſhnou, Burnou, the great kingdom near the Niger. Meanwhile he has ſent from Tripoli, by another conveyance, not yet arrived, the journal of his preſent travels; and there is every reaſon to hope that he will accompliſh his great undertaking of viſiting the unknown central regions of Africa, eſpecially from the following occurrences mentioned in his letter:—He was followed from Sewah by a large party ſent to ſeize him, on ſuſpicion of his being a French ſpy. But his manners and behaviour were ſo completely Moſlem, and he approved himſelf ſo thoroughly maſter of the Koran, that he was releaſed with bleſſings and alms as a good muſſulman, and ſent forward on his journey.

CEMENTING OF BROKEN GLASS.

C. Pajot, of Charmes, lately tranſmitted to the Philomatic Society and the National Inſtitute, ſmall bits of glaſs of different qualities, which he had joined and ſoldered ſo firmly that the glaſs would rather break cloſe to the joining than in the actual place. The form of the fracture does not at all prevent the operation. The line of junction is ſcarcely diſcernible, and in ſome places not viſible. C. Pajot has not made his proceſs known.

C. Swediaur informed the Society, that a perſon named Hollenweger, about twelve or fourteen years ago, had in his preſence, and that of Lavoiſier and Meunier, performed ſome experiments, by means of which he joined in a ſolid manner, ſo as to make the junction hardly viſible, fragments of blown glaſs.

C. Chaptal ſaid alſo that ſome perſon had ſhewn him, about fifteen years ago, a glaſs bottle, the neck of which was ſo perfectly ſoldered to the glaſs ſtopper, that, when cut through, the circle of junction was ſcarcely perceptible. The bottle

bottle contained liquor of flints, and had lain on its ſide for a long time. "After ſeeing this fact, ſaid Chaptal, I conceived the poſſibility of ſoldering together two plates of glaſs. I explained my ideas on this ſubject, and ſhewed the bottle, in my public lectures; and I am of opinion, that, by gradually withdrawing the diſſolving alkali, it might be poſſible to unite ſuch plates."

Before we diſmiſs this article, we beg leave to remark, that there appears to be nothing ſo wonderful in joining broken pieces of glaſs, in the way above deſcribed, as to juſtify Pajot in concealing the proceſs. A little reaſoning will lead any one to it. All that is neceſſary is to interpoſe, between the parts, a glaſs ground up like a pigment, but of eaſier fuſion than the pieces to be joined, and then expoſing them to ſuch a heat as will fuſe the cementing ingredient, and make the pieces agglutinate without being themſelves fuſed. A glaſs fit for the purpoſe of cementing broken pieces of flint glaſs, may be made by fuſing ſome of the ſame kind of glaſs, previouſly reduced to a powder, along with a little red lead and borax, or with the borax only.

DIED.

At Edinburgh, on the 6th inſt. Dr. Joſeph Black, one of the Phyſicians to his Majeſty for Scotland, Profeſſor of Chemiſtry in the Univerſity of Edinburgh, and a Member of the different reſpectable Medical and Literary Societies in Europe. The noble ſcience of Chemiſtry has received many valuable improvements and elucidations from the genius and induſtry of the learned Dr. Black, which will tranſmit his name with honour to the lateſt poſterity.

His papers were found in ſuch perfect order by his executors, that we underſtand they intend ſpeedily to publiſh them, with a life of the author.

Errata in our Laſt.

In the deſcription of Mr. Howard's Furnace, laſt line, (p. 192,) for *Windſor* read *fire* bricks.

Page 198, line 26, for *tvpani* read *tympani*.

In the deſcription of the monument to Count Rumford, p. 206, line 13, read

At the creative glance of Charles Theodore,
Rumford, the friend of mankind, &c.

THE

PHILOSOPHICAL MAGAZINE.

JANUARY 1800.

I. *A Communication from* GEORGE PEARSON, *M. D. F. R. S.* &c. *Phyſician to St. George's Hoſpital,* &c. *concerning the Eruptions reſembling the Small-Pox, which ſometimes appear in the Inoculated Vaccine Diſeaſe.*

FOR reaſons, the explanation of which would lead into too long a detail, and which, indeed, cannot be given with perfect propriety in this ſheet; I feel myſelf compelled to publiſh ſome obſervations concerning the eruptions which appear, in ſome inſtances, in the cow-pock by inoculation.

Although the new inoculation in the preſent year has been, I think, ſufficiently extenſive to manifeſt the advantages of it over that for the ſmall-pox, ſo that it is not likely to be ever totally laid aſide; yet the unexpected appearance of eruptions has inclined many perſons to be of opinion that no beneficial conſequences can be produced by this practice, or, at leaſt, that ſuch conſequences at beſt ſeem to be problematical. It may be uſeful to obſerve, that ſome of the Advocates for the cow-pock inoculation contend, that eruptions are never produced by it; accordingly, they aſſert, that, in theſe eruptive caſes, the diſorder was not the cow-pock, but the ſmall-pox; the variolous poiſon having either been inſerted inadvertently, or the conſtitution having been af-

 fected

fected by it caſually. To juſtify what is advanced, it is incumbent on the Aſſertors either to prove that ſuch errors have been committed, or at leaſt they ought to be able to oppoſe equally extenſive experience to that of the adverſe party: for no ſound reaſoner will conſider opinions, which are only founded in conjecture, to be demonſtrated truths. I will not, however, take upon myſelf the unrequired taſk of attempting to vindicate others from the above charges; but I ſhall only perform a duty in ſtating the reſult of my own experience with regard to the point in queſtion; conceiving that by this means the truth may be brought to light, if aided by the evidence from experience of future inquirers.

In the courſe of my practice the latter end of February, and in March following, I diſtinctly recollect four caſes in which I firſt ſaw eruptions from the vaccine inoculation reſembling ſo much thoſe of the ſmall-pox, that I ſhould not have heſitated to conſider them as belonging to this diſeaſe, if I had not excited them by a different poiſon from the variolous. I obſerved, however, at that time, ſome appearances of theſe eruptions different from thoſe which uſually occur in the ſmall-pox. Almoſt all the eruptions, in the ſtage of deſiccation, afforded ſhining, ſmooth, black or reddiſh-brown ſcabs; very few of them having previouſly ſuppurated. Finding, in two other inſtances, that the matter from the inoculated puſtule of theſe patients produced a ſimilar eruptive diſorder, and alſo the ſame being the event in the practice of two or three of my correſpondents, whom I had furniſhed with matter from the above eruptive caſes, I from that time uſed matter only from the caſes in which no eruptions appeared. After this precaution, no eruptive caſes reſembling the ſmall-pox occurred in my practice during the whole of laſt ſummer and the preſent winter. I ſay, no caſes occurred reſembling the ſmall-pox; but certainly eruptions, in number from a ſingle one to about a dozen, which were large, red, hard pimples, with little or no lymph, and never with any pus, occurred, probably, in one caſe out of twenty or thirty. Theſe ſpots, ſo unlike the ſmall-pox, produced no trouble; and were of ſuch a ſhort duration, that, when I ſpeak of eruptions, I do not include them in the number; I include in the account thoſe only in which the

eruptions reſembled the ſmall-pox: nor do I reckon among the eruptive caſes thoſe in which, now and then, a raſh broke out about the 14th day after inoculation; and which was as troubleſome as the *Urticaria*. My experience, then, with reſpect to the caſes of eruptions being diminiſhed in number by avoiding inoculation with matter of ſimilar eruptive caſes, coincides with Dr. Woodville's; and confirms what he has already ſo uſefully communicated to the public. It was obvious to ſuſpect, on the firſt occurrence of the eruptive caſes, that variolous matter, in an unobſerved way, but from ſources which could not even be conjectured, had been introduced into the conſtitution inſtead of the vaccine poiſon. This conjecture, in ſpite of the cleareſt evidence of ſenſe reſpecting the nature of the matter uſed, received ſome countenance from the non-appearance of eruptions as above ſtated; but, from the occurrence of ſuch caſes, in the practice of other Inoculators, in the laſt autumn and this winter, I think it is very unreaſonable to doubt any longer, that, either on account of peculiar ſtates of the human animal œconomy, or on account of ſome co-operating agents, the genuine vaccine poiſon does now and then produce a certain variety of the cow-pock, characteriſed by the appearance of puſtules, like thoſe of the variola. I have good evidence alſo to ſhew, that even in the hands of thoſe very Inoculators, who a little time ago would not allow that the vaccine poiſon could produce eruptions, ſuch caſes have lately occurred.

In the month of October laſt I inoculated a child two years of age with the vaccine poiſon. The original matter, which had produced this matter, I took from the cow in March laſt; ſince which time the vaccine diſeaſe had been excited by it, in my hands, in a great number of patients. The vaccine diſeaſe took place with the uſual appearances, in the inoculated part, and affected the whole conſtitution in the ordinary manner; but a few eruptions broke out on the ſecond or third day after the ſlight fever; they were, however, only the red large pimples above mentioned, not at all like the ſmall-pox. Mr. Keate carried matter from this child to Brighthelmſtone, where Mr. Barret inoculated *two* children, who took the diſeaſe; and from one of theſe Mr. Keate ino-

culated *three* children. They all had the uſual fever about the eighth day, and all had a number of eruptions, except one, which had only five or ſix, and theſe dried on the fifth day. This laſt caſe was probably that which Mr. Keate informs me had, in the inoculated part, the genuine vaccine puſtule; but in all the others Mr. Barret obſerved that in the inoculated part the puſtule was ragged at the edges, and flat, more reſembling the variolous puſtule. Matter from theſe patients was ſent to Petworth, where Mr. Andre informs me he inoculated with it *fourteen* children. They all took the diſeaſe, and had eruptions like the variolous. *Three* children at the breaſt had from *three* to *twelve puſtules*. The remaining *eleven* children had from fifty to ſeveral hundred eruptions. The ſtate of the arms, and the characters of the puſtules of the inoculated part, are not mentioned. None of the above patients died, nor is any mention made by Mr. Keate, Mr. Barret, and Mr. Andre, even of any apprehenſion of danger. Dr. Thornton ſent me lately a caſe with eruptions, produced by matter which I originally took from a cow. According, then, to experience, we draw theſe concluſions:— 1. That in certain conſtitutions, or under the circumſtances of certain co-operating agents, the vaccine poiſon produces a diſeaſe reſembling the ſmall-pox; and of courſe the puſtule in the inoculated part is very different from that of the vaccine pock ordinarily occurring, and the eruptions reſemble very much, if not exactly, ſome varieties of the ſmall-pox. 2. That in ſome inſtances theſe eruptions have occurred although the inoculated part exhibited the genuine vaccine puſtule. 3. That the matter of ſuch eruptive caſes, whether taken from the inoculated part, or from other parts, produces univerſally, or at leaſt generally, ſimilar eruptive caſes, and has not, I believe, been ſeen to go back, by paſſing through different conſtitutions, to the ſtate in which it produces what is called the genuine vaccine diſeaſe. 4. That eruptions, of a different appearance from variolous ones, ſometimes occur in the true cow-pock.

Now, whether the vaccine poiſon, when it produces theſe caſes reſembling the ſmall-pox, has really become, by compoſition or decompoſition, variolous matter, is undetermined.

If

If this ſhould be found to be the caſe by future experiments, ſtill we muſt conſider the two poiſons as of diſtinctly different ſpecies, on account of the different characters of the puſtule in the ſmall-pox and cow-pox; although, as juſt ſaid, by the combination of ſome other ſubſtance with the cow-pox poiſon, or by the ſeparation of ſome one of the conſtituent ingredients of this poiſon, the variolous poiſon may be produced. To illuſtrate this theory let it be conſidered, that magneſia and ſulphate of magneſia are different ſpecies of ſubſtances; although they agree in ſome of their principal effects on the human conſtitution, and in other properties; but, by the union of magneſia with ſulphuric acid, it becomes ſulphate of magneſia. Or the illuſtration may be given converſely. As, then, we have diſtinct denominations for theſe two ſubſtances, ſo we ought to have them for the two poiſons, and the two different diſeaſed ſtates they produce, namely, the cow-pox and the ſmall-pock. Accordingly, Dr. Odier, of Geneva, whoſe powers as a Dialectician, and whoſe acuteneſs as a Philologer, I can atteſt from the period of his academical ſtudies, has "baptiſed" (to uſe the language of Dr. De Carro,) the new diſeaſe *La Vaccine*, or *Vaccina*, rejecting the abſurd name of the Engliſh, *variolæ vaccinæ*.

But, to return to the immediate queſtion under diſcuſſion, granting that eruptions are liable to be produced by the inoculation of the cow-pock, what difference ought this accident to make in our eſtimate of the value of the new practice, from the eſtimate on the ſuppoſition that no ſuch eruptions would occur? I apprehend the value is hereby depreciated, but not to ſuch a degree as to create any reaſonable apprehenſions of the failure of the Vaccine Inoculation in ſuperſeding and extinguiſhing the ſmall-pox. Becauſe,

1. If the precaution be taken of avoiding the inoculation with matter from eruptive caſes, as far as I have ſeen, not above one caſe, with variolous-like eruptions, will be produced among 200 inſtances of inoculated cow-pox.

2. Theſe eruptive caſes are, as far as I have obſerved, like the ordinary kinds of inoculated ſmall-pox.

3. I have ſeen no diſfigurations of the ſkin from this variety of cow-pox; but I think it juſt to acknowledge that, from

from the experience I have had, no practitioner can anſwer for ſuch caſes not occurring in any inſtance, and as danger is always in proportion to the number of eruptions, which number no one can pretend to limit, it is evident that the chance of life during this diſeaſe is leſſened, although but a very little. Provided this ſtatement be made to the patient, it does not appear to me that the fact of the liability to eruptions ought to impede the progreſs of the Vaccine Inoculation; but if, on the contrary, the patient is aſſured that ſuch eruptions will not occur, there is good ground for the practice falling into diſcredit, or at leaſt for many perſons, with reaſon, being diſcontented.

After this repreſentation of an unfavourable part of the hiſtory of the cow-pock, it is conſolatory to be able to counterpoiſe it with ſome new facts, which, like that of the eruptions, have been diſcovered in the courſe of further experience. It now appears that a perſon who has had the ſmall-pox, is not ſuſceptible of the cow-pox by inoculation *; nor is a perſon ſuſceptible of the conſtitutional affection from the cow-pox poiſon more than once. On the whole, then, we have gained perhaps as much as we have loſt ſince the publication of the original account: and unleſs ſome new adverſe facts ſhall be diſcovered, and confiding that the public will adopt a practice which is ſo manifeſtly to their intereſt, the change effected in the practice of medicine will be ſo eminently memorable, *that the introduction of the Vaccine Inoculation muſt become an epoch in the hiſtory of phyſic.*

II. *Obſervations reſpecting the Earthquake which took place in Peru in the year* 1797. *By* M. Cavanilles †.

THERE are reckoned to be more than ſixteen volcanoes in the kingdom of Quito which are in a continual ſtate of eruption, and which throw up thick vapours, often mixed

* The caſes of milkers with chopt hands being repeatedly affected by the cow-pox poiſon in the *caſual way*, whether they had undergone the ſmall-pox or not, probably occaſioned the error of the concluſions here alluded to.

† From the *Journal de Phyſique*, Fructidor an. 7.

with

with flames, either from their craters or through their lateral fiſſures. In the midſt of the moſt profound calm there is frequently heard a dreadful bellowing noiſe, the forerunner of earthquakes, to which this part of the world is often expoſed. After the year 1791, this noiſe was frequently heard in the neighbourhood of the mountain of Tunguragua. Antonio Pineda and Née, the two naturaliſts employed in the expedition round the world, when examining the declivity of this volcano, the lava of which had been hardened more by the internal fire than by the ardour of the ſun, were ſtruck with terror by the horrible ſound which they heard, and the heat which they experienced. Pineda, that valuable member of ſociety, whoſe premature death is ſtill deplored by the friends of ſcience, foretold that a terrible eruption was preparing in the mountain of Tunguragua; and his conjectures were confirmed by the event. On the 4th of February 1797, at three quarters paſt ſeven in the morning, the ſummit of the volcano was more free from vapours than uſual; the interior part of the mountain was agitated by frequent ſhocks, and the adjacent chains burſt in ſuch a manner, that in the ſpace of four minutes an immenſe tract of country was convulſed by an undulating movement. Never did hiſtory relate the effects of an earthquake ſo extraordinary, and never did any phenomenon of Nature produce more misfortunes, or deſtroy a greater number of human beings. A number of towns and villages were deſtroyed in a moment: ſome of them, ſuch as Riobamba, Quero, Pelileo, Patate, Pillaro, were buried under the ruins of the neighbouring mountains; and others in the juriſdictions of Harnbata, Latacunga, Guaranda, Riobamba, and Alauſi, were entirely overthrown. Some ſuſtained prodigious loſs by the gulphs which were formed, and by the reflux of rivers intercepted in their courſe by mounds of earth; and others, though in part ſaved, were in ſuch a ſhattered ſtate as to threaten their total ruin. The number of perſons who periſhed during the firſt and ſucceeding ſhocks are eſtimated at 16,000. At ten o'clock in the morning, and four in the afternoon, the ſame day, (February 4,) after a dreadful noiſe, the earth was again agitated with great violence, and it did not ceaſe to ſhake, though

though faintly, for the whole month of February and March; but, at three quarters paſt two in the morning of the 5th of April, the villages already ruined were again expoſed to ſuch violent ſhocks as would have been ſufficient to deſtroy them. This extraordinary phenomenon was felt throughout the extent of 140 leagues from eaſt to weſt, from the ſea as far as the river Napo; and without doubt farther, for we are little acquainted with theſe diſtricts which are inhabited by the ſavages. The diſtance north-eaſt and ſouth-weſt between Popajan and Piura, is reckoned to be 170 leagues; but in the centre of that diſtrict, 1 degree 16·6 from theſe places, is ſituated the part totally deſtroyed, and which comprehends forty leagues from north to ſouth between Guarandam and Machache, and twenty leagues from eaſt to weſt. But, as if an earthquake alone had not been ſufficient to ruin this fertile and populous country, another misfortune, hitherto unknown, was added. The earth opened, and formed immenſe gulphs; the ſummits of the mountains tumbled down into the valleys, and from the fiſſures in their ſides there iſſued an immenſe quantity of fetid water, which in a little time filled up valleys a thouſand feet in depth and ſix hundred in breadth. It covered the villages, buildings, and inhabitants; choaked up the ſources of the pureſt ſprings, and, being condenſed by deſiccation, in the courſe of a few days, into an earthy and hard paſte, it intercepted the courſe of rivers, made them flow backwards for the ſpace of eighty-ſeven days, and converted whole diſtricts of dry land into lakes. Very extraordinary phenomena, which will doubtleſs be one day mentioned in hiſtory, occurred during theſe earthquakes; I ſhall, however, content myſelf with mentioning only two of them. At the ſame moment that the earth ſhook, the lake of Quirotoa, near the village of Inſiloc, in the juriſdiction of Lacatunga, took fire, and the vapour which roſe from it ſuffocated the cattle and flocks that were feeding in the neighbourhood. Near the village of Pelileo, a large mountain named Moya, which was overturned in an inſtant, threw out a prodigious ſtream of the before-mentioned thick fetid matter, which deſtroyed and covered the miſerable remains of that city. Naturaliſts will

one

one day find, in theſe ravaged countries, objects worthy of their reſearches. Fragments of the minerals and earths of Tunguragua are about to be tranſported to Spain: but it is not in ſuch fragments that we ought to ſearch for the cauſe of theſe ſurpriſing phenomena; we muſt viſit the country itſelf, where this conflict of the elements took place, and where the ruins it occaſioned are ſtill to be ſeen *.

III. *Experiments to determine the Quantity of Tanning Principle and Gallic Acid contained in the Bark of various Trees. By* George Biggin, *Eſq.* †

THE bark of trees contains the aſtringent principle called gallic acid, and alſo that principle which has a peculiar affinity to the matter of ſkin, and which, from the uſe to which it is applied, is called the tanning principle. But, in the preſent mode of tanning, bark is applied in *maſs* to the ſkins; conſequently, *both* principles are applied. It remains for examination, whether both principles are uſeful in the proceſs of tanning; for, if they are not both uſeful, probably *one* is detrimental.

To a nobleman, whoſe zeal on every occaſion by which the ſciences or arts may receive illuſtration or improvement, is eminently conſpicuous, and to whoſe public energy, as well as private friendſhip, I feel myſelf much indebted, to his Grace the Duke of Bedford, I owe the means of proſecuting ſome experiments on this ſubject. His Grace, by collecting a variety of barks, at Woburn, gave me an opportunity of making ſome experiments to aſcertain the quantity of tanning principle and gallic acid each bark contained. For that purpoſe I made uſe of the following methods, according to the principles laid down by M. Seguin:—

By diſſolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which, as it contains the matter of ſkin in ſolution, is a teſt for the

* The volcano of Tunguragua occaſioned an earthquake in 1557.

† From the *Philoſophical Tranſactions of the Royal Society of London.*

tanning principle. By a faturated folution of fulphat of iron, I obtained a teft for the gallic acid.

I then took one pound of the bark I meant to try, ground as for the ufe of tanners, and divided it into five parts, each part being put into an earthen veffel. To one part of this bark I added two pounds of water, and infufed them for one hour. Thus I procured an infufion of bark, which I poured on the fecond part of the bark, and this ftrengthened infufion again on the third part, and fo on to the fifth. But, as a certain portion of the infufion will remain attached to the *wood* of the bark after the infufion is poured or drawn off, I added a third pound of water to the firft part, and then followed up the infufion on the feveral parts till the three pounds of water, or fo much of them as could be feparated from the bark, were united in the fifth veffel; from which I generally obtained about one pint of ftrong infufion of bark *.

To a certain quantity of this infufion, I added a given meafure of the folution of glue, which formed an immediate precipitate, that may be feparated from the infufion by filtering paper. When dried, it is a fubftance formed by the chemical union of the matter of fkin with the tanning principle, and is, in fact, a powder of leather. By faturating the infufion with the folution of glue, the whole of the tanning principle may be feparated by precipitation.

For the Gallic Acid.

To the pound of bark left in the earthen veffels, and already deprived of its tanning principle by thefe *quick* infufions, I added a *given* quantity of water, to procure a ftrong infufion of the gallic acid, which requires a longer time, (fay forty-eight hours.) This infufion, when obtained pure †, affords little figns of the prefence of the tanning principle,

* The fpecific gravity of this infufion was afcertained by an hydrometer whofe gradations are inverfe to thofe of a fpirit hydrometer.

† It is hardly poffible, from the intimate connection of the two principles, to feparate them entirely by infufion: in the infufion of tanning principle, there will always exift a little gallic acid; and, in an infufion of gallic acid, a little tanning principle will commonly be prefent, unlefs the infufion of gallic acid is very weak, and procured by a third or fourth watering.

when

when tried by the teſt of the ſolution of glue; but, with the ſolution of ſulphat of iron, it gives a ſtrong black colour, (the common black dye,) which differs in denſity according to the quality of the bark: this may be further proved, by boiling a ſkain of worſted in the dye, by which the gradations of colour will be very perceptibly demonſtrated.

Having thus obtained a point of compariſon; by making a ſimilar infuſion, under ſimilar circumſtances, of any bark, or vegetable ſubſtance, and paying ſtrict attention to the ſpecific gravity of the infuſion, the quantity of precipitate of leather, and the denſity of colour produced by given quantities of one or the other teſt, the reſult will be, a comparative ſtatement of the reſpective powers of any bark, or vegetable ſubſtance. This comparative ſtatement I conceive to be ſufficient for all commercial purpoſes.

As oak bark is the uſual ſubſtance employed in the trade of tanning, if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined, by comparing its quantity of tanning principle and price with thoſe of oak bark.

For an accurate chemical analyſis, I have tried a variety of acids, and ſimple and compound affinities; and, having purſued the above experiments at the ſame time that I was employed on ſome in dyeing, I found the muriat of tin (the method of uſing which is deſcribed by Mr. Prouſt in the *Annales de Chimie*,) very convenient. A ſolution of it being added to the infuſion of bark, forms a precipitate with the tanning principle, leaving the gallic acid ſuſpended: the precipitate is of a fawn colour, and is compoſed of tanning principle and oxydated tin.

By theſe means I have been enabled to form a comparative ſcale of barks; which, however, I do not produce as accurate. Oak bark, in its preſent ſtate, as procured for commercial purpoſes, differs very much in quality, from accidental circumſtances: the ſeaſon of the year in which it is collected occaſions a ſtill more important difference, conſequently the ſcale now produced muſt be very imperfect; but I am of opinion, that, by the purſuits of ſcientific men,

who may be inclined to inveſtigate this ſubject more fully, a very accurate ſcale may hereafter be formed.

In the following ſcale, I have taken Sumach as the moſt powerful in the comparative ſtatement; leaving, however, a few degrees for a *ſuppoſed maximum of tanning principle*, which I reckon twenty.

SCALE OF BARKS.

Bark of	Gallic acid, by colour.	Tanning principle, by hydrometer.	Tanning principle, (in grains,) from half a pint of infuſion and an ounce of ſolution of glue.
Elm * - - -	7	2,1	28
Oak, cut in winter	8	2,1	30
Horſe cheſtnut -	6	2,2	30
Beech - - -	7	2,4	31
Willow (boughs)	8	2,4	31
Elder - - -	4	3,0	41
Plum-tree - -	8	4,0	58
Willow (trunk) -	9	4,0	52
Sycamore - -	6	4,1	53
Birch - - -	4	4,1	54
Cherry-tree - -	8	4,2	59
Sallow - - -	8	4,6	59
Mountain aſh -	8	4,7	60
Poplar - - -	8	6,0	76
Hazel - - -	9	6,3	79
Aſh - - - -	10	6,6	82
Spaniſh cheſtnut -	10	9,0	98
Smooth oak - -	10	9,2	104
Oak, cut in ſpring	10	9,6	108
Huntingdon or Leiceſter willow -	10	10,1	109
Sumach - - -	14	16,2	158

It is to be obſerved, that the barks do not keep any reſpective proportion in the quantity of gallic acid and tanning principle contained in each; which is an evidence of the

* The infuſion of elm was ſo loaded with mucilage that it was with difficulty I could ſeparate the tanning principle, or try the ſpecific gravity.

diſtinctneſs

diſtinctneſs of principle, and may perhaps open a new field for ſaving oak bark in dyeing, as the willows, ſallow, aſh, and others, produce a very fine black. It is alſo worthy of obſervation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions between the winter and ſpring felled oaks. This fact may lead to the diſcrimination of the proper time for cutting; which is, probably, when the ſap has completely filled and dilated that part of the vegetable intended for uſe. This will make a difference in the ſeaſon of cutting oak, elm, and other trees, ſhrubs, &c. Leaves ſhould be taken when arrived at their full ſize, and then dried under cover; for, as the tanning principle is ſo ſoluble, and the ſubſtance that contains it ſo thin, (in a leaf,) the dew alone might diſſolve it.

Finally, as the gallic acid does not ſeem to combine with the matter of ſkin, and as its aſtringency will corrugate the ſurface, we may, I think, conclude, that its preſence in tanning is not only uſeleſs, but detrimental.

IV. *Extract of a Memoir on the Grecian Method of dyeing Cotton Yarn Red.* By C. Felix*.

THAT beautiful red dye given to cotton in the Ottoman empire, is known in Europe under the name of Turkey red, Levant red, or Adrianople red. As it is believed among us that this colour reſults chiefly from the proceſſes employed in the dyeing, I ſhall give an account of thoſe followed in the Grecian manufactories. It muſt, however, be obſerved, that in theſe manufactories the workmen dye at one time a maſs of ſkains weighing thirty-five occas; each occa being equal to about fifty ounces.

The firſt proceſs is that of cleaning the cotton, for which purpoſe three leys are employed; one of ſoda, another of aſhes, and a third of lime. The cotton is thrown into a tub, and moiſtened with the liquor of the three leys in equal quantities; it is then boiled in pure water, and waſhed in running water.

* From the *Annales de Chimie*, No. 92.

The

The ſecond bath given to the cotton is compoſed of ſoda and ſheep's dung diſſolved in water. To facilitate the ſolution the ſoda and dung are pounded in a mortar. The proportions of theſe ingredients employed, are, one occa of dung, ſix of ſoda, and forty of water. When the ingredients are well mixed, the liquor expreſſed from them is ſtrained, and being poured into a tub, ſix occas of olive oil are added to it, and the whole is well ſtirred till it becomes of a whitiſh colour, like milk. The cotton is then beſprinkled with this water, and when the ſkains are thoroughly moiſtened, they are wrung, preſſed, and expoſed to dry. The ſame bath muſt be repeated three or four times, becauſe it is this liquor which renders the cotton more or leſs fit for receiving the dye. Each bath is given with the ſame liquor, and ought to continue five or ſix hours. It is to be obſerved that the cotton, after each bath, muſt be dried without being waſhed, as it ought not to be rinſed till after the laſt bath. The cotton is then as white as if it had been bleached in the fields.

The bath of ſheep's dung is not uſed in our manufactories; it is a practice peculiar to the Levant. It may be believed that the dung is of no utility for fixing the colours; but it is known that this ſubſtance contains a great quantity of volatile alkali, in a diſengaged ſtate, which has the property of giving a roſy hue to the red. It is therefore probable that it is to this ingredient that the red dyes of the Levant are indebted for their ſplendour and vivacity. This much, at any rate, is certain, that the Morocco leather of the Levant is prepared with dog's dung; becauſe it has been found that this dung is proper for heightening the colour of the lack. The bath of dung is followed by the proceſs of galling.

The galling is performed by immerſing the cotton in a bath of warm water, in which five occas of pulveriſed gall-nuts have been boiled. This operation renders the cotton more fit for being ſaturated with the colour, and gives to the dye more body and ſtrength. After the galling comes alumming, which is performed twice, with an interval of two days, and which conſiſts in dipping the cotton into a bath of water in which five occas of alum have been infuſed, mixed with five occas of water alcaliſed by a ley of ſoda. The aluming

muſt

muſt be performed with care, as it is this operation which makes the colouring particles combine beſt with the cotton, and which ſecures them in part from the deſtructive action of the air. When the ſecond aluming is finiſhed, the cotton is wrung; it is then preſſed, and put to ſoak in running water, after being incloſed in a bag of thin cloth.

The workmen then proceed to the dyeing.—To compoſe the colours they put in a kettle five occas of water and thirty-five occas of a root which the Greeks call *ali-zari*, or painting colour, and which in Europe is known under the name of *madder*. The madder, after being pulveriſed, is moiſtened with one occa of ox or ſheep's blood. The blood ſtrengthens the colour, and the doſe is increaſed or leſſened according to the ſhade of colour required. An equal heat is maintained below the kettle, but not too violent; and when the liquor ferments, and begins to grow warm, the ſkains are then gradually immerſed before the liquor becomes too hot. They are then tied with packthread to ſmall rods, placed croſſwiſe above the kettle for that purpoſe, and when the liquor boils well, and in an uniform manner, the rods from which the ſkains were ſuſpended are removed, and the cotton is ſuffered to fall into the kettle, where it muſt remain till two-thirds of the water is evaporated. When one-third only of the liquor remains, the cotton is taken out and waſhed in pure water.

The dye is afterwards brought to perfection by means of a bath alcaliſed with ſoda. This manipulation is the moſt difficult and the moſt delicate of the whole, becauſe it is that which gives the colour its tone. The cotton is thrown into this new bath, and made to boil over a ſteady fire till the colour aſſumes the required tint. The whole art conſiſts in catching the proper degree: a careful workman, therefore, muſt watch with the utmoſt attention for the moment when it is neceſſary to take out the cotton, and he will rather burn his hand than miſs that opportunity.

It appears that this bath, which the Greeks think of ſo much importance, might be ſupplied by a ley of ſoap; and it is probable that ſaponaceous water would give the colour more brightneſs and purity.

When

When the colour is too weak, the Levantines know how to ſtrengthen it by increaſing the doſe of the colouring ſubſtances; and when they wiſh to give it brightneſs and ſplendour, they employ different roots of the country, and, in particular, one named *faſſari*, ſpecimens of which I have ſent to France.

The ali-zari, which is the principal colouring matter employed in the Greek dye-houſes, is collected in Natolia, and is brought to Greece from Smyrna: ſome of it comes alſo from Cyprus and Meſopotamia. The ſuperiority of this Levantine plant to the European madder is acknowledged by all thoſe acquainted with the art of dyeing, and may ariſe from two cauſes; the manner in which it is cultivated, and the method employed for its deſiccation.

As the ali-zari appears to be of a weaker conſtitution than the common madder: as its branches are more delicate, its leaves ſmoother and tenderer, and its ſtem more fragile, it is ſupported by ſticks, as peaſe are among us. A well nouriſhed ſtem acquires in this manner more conſiſtence, and ſends forth more roots. It is to be obſerved, that it is the woody part of the roots which affords the greateſt quantity of colouring particles. The ali-zari is not collected till the fifth or ſixth year, that is to ſay, when it has acquired its full ſtrength; while in France and Zealand, where land is of more value than in the Levant, the inhabitants wiſh to reap too ſoon, and collect the madder before it comes to complete maturity.

The method employed in the deſiccation contributes alſo to improve the quality of the ali-zari. The Levantines dry it in the open air; and this operation is eaſy in a country where great dryneſs prevails in the atmoſphere, while, in our damp climates, we are obliged to dry the madder by ſtoves. Hence it happens that the ſmoke, which mixes itſelf with the cold air, and penetrates the roots, impregnates them with fuliginous particles, which alter the colouring ſubſtance; an accident which does not take place when the madder is dried without the aſſiſtance of fire. It is, however, poſſible, that the ali-zari and madder may never produce the ſame tone of colour, notwithſtanding all the care that may be employed

in the culture and drying of the latter; becauſe there may be as much difference between theſe two plants, ſo like to each other, as between the goats of France and thoſe of Angora.

It is probable, alſo, that the ſuperiority of the ali-zari over madder may ariſe only from its ſtem being tenderer, and by its conſequently having a greater diſpoſition to transform itſelf into ſucculent roots. In that caſe, ſeveral of our indigenous plants of the family of the *rubiæ* might be ſubſtituted in its place with advantage; ſuch as the *gallium luteum* and *gallium flore albo*, found in abundance on the hills of Poitou, and the madder, which grows on both ſides of the Alps, and which has been diſtinguiſhed by the name of *rubia lævis Tourinenſium*. All theſe plants give a red as beautiful and as pure as the beſt madder; but I do not know whether the quantity of their colouring matter is equal to their beautiful quality.

The chief manufactories for dyeing ſpun cotton red, eſtabliſhed in Greece, are in Theſſaly. There are ſome at Baba, Rapſani, Tournavos, Lariſſa, Pharſalia, and in all the villages ſituated on the ſides of Oſſa and Pelion. Theſe two mountains may be conſidered as the alembics that diſtil the eternal vapours with which Olympus is crowned, and which diſtribute them throughout the beautiful valleys ſituated around them. Of theſe valleys, that of Tempe has at all times been diſtinguiſhed by the beauty of its ſhady groves and of its ſtreams. Theſe ſtreams, on account of their limpidneſs, are very proper for dyeing, and ſupply water to a great number of manufactories, the moſt celebrated of which are thoſe of Ambelakia.

Ambelakia, on account of the activity which prevails in it, has a greater reſemblance to a town of Holland than a village of Turkey. This village, by its induſtry, communicates life and activity to all the neighbouring country, and gives birth to an immenſe trade, which connects Germany with Greece in a thouſand ways. Its population, which has been tripled within theſe fifteen years, amounts at preſent to 4000, and all theſe people exiſt by dyeing. None of thoſe vices or cares produced by idleneſs are known here. The hearts of

the inhabitants are pure, and their countenances unclouded. Servitude, which degrades the countries watered by the Peneus, has not yet afcended to thefe hills: no Turk can refide or live among thefe people; and they govern themfelves, like their anceftors, by their *protoyeros* and their own magiftrates. Twice have the favage muffulmans of Lariffa, envious of their eafe and happinefs, attempted to fcale their mountains in order to plunder their houfes; and twice have they been repulfed by hands which fuddenly quitted the fhuttle to affume the mufket.

All hands, and even thofe of the children, are employed in the dye-houfes of Ambelakia; and while the men dye the cotton, the women are fpinning and preparing it. The ufe of wheels is not known in this part of Greece; all the cotton is fpun on a diftaff: the thread, indeed, is certainly not fo round or equal, but it is fofter, more filky, and more tenacious; it is lefs apt to break, and lafts longer; it is alfo more eafily whitened, and more proper for being dyed. It is a pleafing fpectacle to fee the women of Ambelakia, each fpinning from a diftaff, and fitting converfing together on the threfhold of their doors; but as foon as a ftranger appears, they inftantly retire and conceal themfelves in their houfes, manifefting, like Galatea, in their precipitate retreat, a defire of flying and of fhewing themfelves:—

Et fugit ad falices, et fe cupit ante videri.

The eye can only have a curfory view of the fhape of fome of thefe women; but it ftill obferves, with furprife, thofe flender and graceful forms of the ancient Grecian females, which ferved as models to the moft beautiful ftatues in the world.

V. *An Account of the Wild Horfes in Spanifh America.* By D. Felix Azara *.

THESE animals were originally carried from Spain by the firft conquerors, and are of the Andalufian breed. They

* From a work, not yet publifhed, on the natural hiftory of Paraguay, by Don Felix Azara, brother of the late Spanifh ambaffador at Paris. This extract is tranflated from the *Decade Philofophique*, No. 9, year 8.

chiefly

chiefly frequent the ſouthern part of the river de la Plata, as far as Rio Negro, the country of the Patagonians, &c. The wild horſes of all theſe countries live in numerous herds, ſome of which, it is ſaid, conſiſt of ten thouſand. As ſoon as they perceive domeſtic horſes in the fields, they run towards them on a full gallop, paſs through the middle of them, or near them, careſs them, and invite them with a kind of grave and prolonged neighing. The domeſtic horſes are ſoon ſeduced, unite themſelves to the independent herd, and depart along with them. It happens not unfrequently that travellers are ſtopped on the road by the effect of this deſertion. To prevent it, they halt as ſoon as they perceive theſe wanderers, watch their own horſes, and endeavour to frighten away the others. In ſuch caſes the wild horſes follow a certain kind of tactics; ſome are detached before, and the reſt advance in a cloſe column, which nothing can interrupt. If they are ſo alarmed as to be obliged to retire, they change their direction, but without ſuffering themſelves to be diſperſed: ſometimes they make a great number of turns around thoſe which they wiſh to ſeduce, in order to frighten them; at other times they retire after making one turn. Theſe manœuvres are not employed during the night, for the wild horſes then make no attempts. The author is ignorant whether any thing of the ſame kind takes place between one herd of wild horſes and another for the purpoſe of recruiting their number. He aſſerts that Buffon is miſtaken in ſaying that theſe wild horſes have more ſtrength and fleetneſs than the domeſtic horſes of the country, and that they do not differ from the latter either in height or ſhape: but he indeed obſerves, that no compariſon can be made between the independent and domeſtic ſtate of theſe animals, as in that country both ſtates are almoſt ſimilar.

Thoſe who poſſeſs *eſtancias*, or paſtures for keeping domeſtic horſes, place in them a certain number of mares, which are never broke or mounted. They remain during their whole lives in a ſtate of perfect liberty; and for every thirty or forty mares there is a ſtallion, which enjoys the ſame independence. They are counted once or twice a week, in order that they may not ſtray from the habitation. But

too little care is employed in the choice of the ſtallions; and this, in all probability, is one of the principal cauſes why theſe horſes, though left at full liberty, are neither ſo beautiful nor ſo good as thoſe of Andaluſia, from which they are deſcended. The induſtry of man improves the breed of thoſe animals which he appropriates to his own uſe, and, by bringing their ſhape and organs to perfection, indemnifies them for that liberty of which he deprives them. Each ſtallion takes poſſeſſion of a ſmall troop of mares, which he keeps collected by preſſing them with his cheſt, and by biting them if they do not obey with ſufficient docility: the mares, on the other hand, remain attached to their ſultans. If two ſtallions fight, the mares do not abandon the conquered for the conqueror, unleſs the former has ſhewn among them a deficiency of vigour.

The fillies remain with their mothers. When it is time to break the colts they are cut, for no perſon mounts a horſe until that operation has been performed. After caſtration, a halter is put upon the animal intended to be broke; he is tied to a ſtake; a ſaddle is placed upon his back, and well girded, but without crupper or breaſt-leather, and a thong is tied round his under-lip, to which is faſtened, on each ſide, a rein, in order to govern him. The horſeman then mounts with large ſpurs, and rides out into the fields. At firſt the young horſe capers and jumps until he is quite fatigued; after which he is brought back to the ſtake. This exerciſe is repeated ſeveral times in the courſe of the ſame day, and is renewed after the interval of ſeveral days until the courſer capers no more. He is then employed as a broke horſe, but with a halter only; a bridle is not put upon him till a year after, at which time he quits the name of *rodomont* to aſſume that of horſe.

As ſoon as the horſes are cut, they are ſeparated from the mares, and put among the animals of the horſe ſpecies uſed for ſervice, which receive no other ſhelter or food than what they find in the fields. They are accuſtomed to live in one canton, which they never quit. Each unites himſelf to a companion, and with ſuch intimacy that inſtances have been known of ſome of them, after running away, having returned

returned more than ſixty leagues to rejoin their old friend. Theſe friends know each other by their neighing, their ſmell, and the noiſe of their pace.

When the proprietors are deſirous to prevent a numerous troop of domeſtic horſes from ſeparating, they place among them a young mare with a ſmall bell, and which is then called the godmother. They all follow her, and they all know and ſeek for each other as members of the ſame ſociety. The ſame effect may be produced by attaching the bell to one of the horſes of the troop.

When the inhabitants have need of horſes, a man on horſeback, bearing a lance, proceeds towards a troop of theſe animals, and drives them into an incloſure formed of palliſades. A horſeman then enters it, and, when he is within reach, entangles the horſe which he wiſhes to catch with a kind of rope; for, though theſe horſes are fit to be mounted, and ſaid to be docile, they will not ſuffer themſelves to be touched with the hand.

From what has been ſaid, it may be readily ſeen that there is really very little difference between the habits of theſe wild horſes and of thoſe which live in a ſtate of domeſticity. It needs excite no wonder, therefore, that there ſhould be very little in regard to their form, ſize, and qualities.

When the inhabitants wiſh to convert ſome of theſe wild horſes into domeſtic ones, people mounted on horſeback proceed towards a troop of the former; and when they approach them they throw ſome of the ropes already mentioned around their legs, ſo that, being prevented from running, they have time to ſecure them. They are then tied to a ſtake, or a tree, not by the four legs, as ſome have ſaid, but with a ſimple halter made of leather. They are left two or three days without food or drink, are afterwards cut, and are then broke in the ſame manner as the domeſtic horſes. The horſe behaves then as if he had never been wild; but it is not true that they loſe all deſire of recovering their liberty. They readily unite with a troop of wild horſes; and how can it be otherwiſe, ſince thoſe even which have been reared in a ſtate of domeſticity have no repugnance to join them?

The proprietors of the *eſtancias*, or paſtures deſtined for

thoſe horſes which have been tamed, endeavour not only to frighten away the wild horſes, but even to exterminate them. With that view they beat the woods in queſt of them, drive them, if poſſible, into ravines, and kill them by means of lances.

The Pampas eat their fleſh, and particularly that of the fillies, colts, and mares; but they ſometimes kill a very fat ſtallion to make a fire with his greaſe and bones, as in the country of Pampa wood is extremely ſcarce.

The Spaniſh author often contradicts Buffon, not only in regard to local obſervations, which is not aſtoniſhing, but alſo in regard to general ideas, ſuch as the influence of climate, &c. D. Felix Azara refutes the aſſertion of the French naturaliſt, who aſcribes more ſtrength and fleetneſs to the wild than to the domeſtic horſes of theſe countries. He even aſſerts, that he has not been correct in ſaying that each wandering troop ſubmit, by common conſent, to a chief, which ſerves as a guide; which regulates and directs their movements; forms them in the order of battle, by files, companies, ſquadrons, &c. The truth, according to the author, is, that each ſtallion appropriates to himſelf as many mares as he can, which he takes care of, and keeps collected; that he combats any other ſtallion which attempts to deprive him of them; and that each wandering troop conſiſts, therefore, of a number of ſmall diſtinct bodies, which ſometimes unite into one.

In the great number of wild troops which the author ſaw, he never obſerved any other prevailing colours than bay, dark brown, and jet black. If it ſometimes happens that a pied or dirty grey coloured individual is ſeen, or one of any other colour, it may with certainty be concluded that it is a domeſtic horſe which has deſerted. According to the author, there are ninety bay for one dark brown horſe; and black horſes are ſo uncommon, that one of them is ſcarcely ſeen in two thouſand. He thence infers, that theſe three colours, bay, dark brown, and jet black, are a primitive mark, which diſtinguiſhes, at leaſt in part, the horſes which recover their liberty; that the firſt horſe and mare which exiſted had one of theſe three colours, and moſt probably the bay, ſince it appears

appears that, among the wild horſes, the black is becoming extinct, and that this will be the caſe alſo with the brown; that, taking the colour as an index, we might ſay, that the beſt breed of horſes is the bay, then the brown, and next the black; all the other colours being inferior, as they are the reſult of more diſtant degradations from the primitive horſe, which muſt have been the moſt perfect. Experience ſeems, in ſome meaſure, to confirm theſe conjectures; for, except in a few caſes, which are of little conſequence, the bays are the moſt eſteemed, and the browns hold the next rank. He obſerves, that in France a prejudice is entertained againſt the laſt-mentioned colour, which he thinks unreaſonable, and which in his opinion ſeems to ſhew that the French, in this reſpect, have not ſo much diſcernment as the Spaniards. Theſe obſervations, and the inferences which the author draws from them, ſeem to weaken the confidence which might be placed in what has been ſaid by Buffon, on the authority of Herodotus, Leo Africanus, and Marco Polo, of wild white horſes ſaid to have exiſted in Arabia and Numidia. We know how ſuſpicious the teſtimony of the ancients is in regard to natural hiſtory, and that the authority of Buffon himſelf has little weight when he gives teſtimonies inſtead of obſerved facts.

VI. *An Account of the Pearl Fiſhery in the Gulph of Manar, in March and April* 1797. *By* HENRY J. LE BECK, *Eſq.**

FROM the accounts of the former pearl fiſheries at Ceylon, it will be found, that none have ever been ſo productive as this year's. It was generally ſuppoſed that the renter would be infallibly ruined, as the ſum he paid for the preſent fiſhery was thought exorbitant when compared with what had been formerly given: but this conjecture in the event appeared ill-founded, as it proved extremely profitable and lucrative.

The farmer this time was a Tamul merchant, who, for

* From the *Aſiatic Reſearches*, Vol. V.

the

the privilege of fiſhing with more than the uſual number of donies or boats, paid between two and three hundred thouſand Porto-novo pagodas; a ſum nearly double the uſual rent.

Theſe boats he farmed out again to individuals in the beſt manner he could, but for want of a ſufficient number of divers ſome of them could not be employed.

The fiſhing, which commonly began about the middle of February, if wind and weather allowed, was this year, for various reaſons, delayed till the end of the month; yet, ſo favourable was the weather, that the renter was able to take advantage of the permiſſion granted by the agreement, to fiſh a little longer than the uſual period of thirty days.

The fiſhery cannot well be continued after the ſetting-in of the ſouthern monſoon, which uſually happens about the 15th of April, as, after that time, the boats would not be able to reach the pearl banks, and the water being then ſo troubled by heavy ſeas, diving would be impracticable; in addition to which, the ſea-weed, a ſpecies of *fucus*, driven in by the ſoutherly wind, and which ſpreads to a conſiderable diſtance from the ſhore, would be an impediment.

Many of the divers, being Roman Catholics, leave the fiſhery on Sundays to attend divine ſervice in their church at Aripoo; but if either a Mahomedan or Hindoo feſtival happens during the fiſhing days, or if it is interrupted by ſtormy weather, or any other accident, this loſt time is made up by obliging the Catholics to work on Sundays.

The fear of ſharks, as we ſhall ſee hereafter, is alſo another cauſe of interruption. Theſe, amongſt ſome others, are the reaſons that, out of two months, (from February till April,) ſeldom more than thirty days can be employed in the fiſhery.

As this time would be inſufficient to fiſh all the banks, (each of which has its appropriate name, both in Dutch and Tamul,) it is carried on for three or four ſucceſſive years, and a new contract annually made, till the whole banks have been fiſhed; after which they are left to recover.

The length of time required for this purpoſe, or from one general fiſhing to another, has not yet been exactly determined; it was therefore a practice to depute ſome perſons to viſit

viſit the banks annually, and to give their opinion, whether a fiſhery might be undertaken with any degree of ſucceſs*?

From various accounts, which I have collected from good authority, and the experience of thoſe who aſſiſted at ſuch examinations, I conjecture, that every ſeven years ſuch a general fiſhery could be attempted with advantage, as this interval ſeems ſufficient for the pearl ſhells to attain their growth: I am alſo confirmed in this opinion by a report, made by a Dutch governor at Jafnas, of all the fiſheries that have been undertaken at Ceylon ſince 1722; a tranſlation of which is to be found in Wolfe's Travels into Ceylon. But the ruinous condition in which the divers leave the pearl banks at each fiſhery, by attending only to the profit of individuals, and not to that of the public, is one great cauſe that it requires twice the above-mentioned ſpace of time, and ſometimes longer, for rendering the fiſhing productive. They do not pay the leaſt attention to ſpare the young and immature ſhells that contain no pearl; heaps of them are ſeen thrown out of the boats as uſeleſs on the beach between Manâr† and Aripoo: if theſe had been ſuffered to remain in their native beds, they would, no doubt, have produced many fine pearls. It might therefore be adviſable to oblige the boat-people to throw them into the ſea again before the boats leave the bank. If this circumſpection, in ſparing the ſmall pearl ſhells to perpetuate the breed, was always obſerved, ſucceeding fiſheries might be expected ſooner, and with ſtill greater ſucceſs: but the neglect of this ſimple precaution will, I fear, be attended with ſimilar fatal conſequences here, as have already happened to the pearl banks on the coaſt of Perſia, South America, and Sweden, where the fiſheries are by no means ſo profitable at preſent as they were formerly.

Another cauſe of the deſtruction of numbers of both old and young pearl ſhells is the anchoring of ſo many boats

* A gentleman, who aſſiſted at one of the laſt viſits, being an engineer, drew a chart of the banks, by which their ſituation and ſize are now better known than formerly.

† Manara, properly Manâr, is a Tamul word, and ſignifies a ſandy river, from the ſhallowneſs of the ſea at that place.

on the banks; almoſt all of them uſed differently formed, clumſy, heavy, wooden anchors, large ſtones, &c. &c. If this evil cannot be entirely prevented, it might, at leaſt, be greatly leſſened, by obliging them all to uſe anchors of a particular ſort, and leſs deſtructive.

This ſeaſon the Seewel Bank only was fiſhed, which lies above twenty miles to the weſtward of Aripoo, oppoſite to the freſh water rivers of Mooſalee, Modragam, and Pomparipoo. It has been obſerved, that the pearls on the north-weſt part of this bank, which conſiſts of rock, are of a clearer water than thoſe found on the ſouth-eaſt, neareſt the ſhore, growing on corals and ſand.

Condatchey is ſituated in a bay, forming nearly a half moon, and is a waſte, ſandy diſtrict, with ſome miſerable huts built on it. The water is bad and brackiſh, and the ſoil produces only a few, widely ſcattered, ſtunted trees and buſhes. Thoſe perſons who remain here during the fiſhery are obliged to get their water for drinking from Aripoo, a village with a ſmall old fort, lying about four miles to the ſouthward. Tigers, porcupines, wild hogs, pangolines, or the *Ceylon armadillos*, are, amongſt other quadrupeds, here common. Of amphibia, there are tortoiſes, eſpecially the *teſtudo geometrica*, and various kinds of ſnakes. A conchologiſt meets here with a large field for his enquiries. The preſents which I made to the people employed in the fiſhery, to encourage them to collect all ſorts of ſhells which the divers bring on ſhore, produced but little effect; as they were too much taken up in ſearching after the mother-of-pearl ſhells to pay attention to any other object. However, my endeavours were not entirely uſeleſs; I will ſpecify here a few of the number I collected during my ſtay: different kinds of *pectines* *, *palium porphyreum*, *ſolen radiatus* †, *Venus caſtrenſis Linn.* ‡ *aſtrea hyotis* §, *oſtr. Forſkolii*, *oſtr. Malleus* ||, *mytilus hirundo Linn.* ¶, *ſpondilus crocius*, *pholas*

* Scallops.
† Radiated razor-ſhell.
‡ Alpha cockle.
§ Double cock's-comb.
|| Hammer oyſter; theſe were pretty large, but many broken, and ſome covered by a calcareous cruſt. It is very probable that among thoſe there may be ſome precious *white* ones.
¶ Swallow muſcle.

pufillus

*puſillus Linn.**, *mitra epiſcopalis Linn.*, *lepas ſtriata Pennanti, (vide Zool. Brit.) patella tricarinata Linn.*, *bulla perfecta maculata* †, *harpa nobilis*, *porcellana ſalita Rumph.* ‡, *ſtrombus ſcorpio*, and other of inferior kinds. Amongſt the *zoophytes*, many valuable ſpecies of *ſpongiæ*, *corallinæ*, *ſatulariæ*, &c. a great variety of ſea-ſtars, and other marine productions, that cannot be preſerved in ſpirits, but ſhould be deſcribed on the ſpot. Theſe, as well as the deſcription of the different animals inhabiting the ſhells, are the more worthy of our attention, and deſerve farther inveſtigation, as we are yet very deficient in this branch of natural hiſtory.

During the fiſhing ſeaſon, the deſert, barren place Condatchey offers to our view a ſcene equally novel and aſtoniſhing. A heterogeneous mixture of thouſands of people, of different colours, countries, caſts, and occupations; the number of tents and huts erected on the ſea-ſhore, with their ſhops or bazars before each of them; and the many boats returning on ſhore in the afternoon, generally richly laden; all together form a ſpectacle entirely new to an European eye. Each owner runs to his reſpective boat as ſoon as it reaches the ſhore, in hopes of finding it fraught with immenſe treaſure, which is often much greater in imagination than in the ſhell; and though he is diſappointed one day, he relies with greater certainty on the next, looking forward to the fortune promiſed him by his ſtars, as he thinks it impoſſible for the aſtrological predictions of his Bráhmen to err.

To prevent riot and diſorder, an officer with a party of Malays is ſtationed here. They occupy a large ſquare, where they have a field-piece, and a flag-ſtaff for ſignals.

Here and there you meet with brokers, jewellers, and merchants, of all deſcriptions; alſo ſuttlers, offering proviſions and other articles to gratify the ſenſual appetite and luxury. But by far the greater number are occupied with the pearls. Some are buſily employed in aſſorting them; for which purpoſe they make uſe of ſmall braſs plates perforated with holes of different ſizes; others are weighing, and offering them to

* The wood-piercer. † Diving ſnail, (Grew, Muſ.)

‡ Salt-coury, Kl.

the purchaſer; while others are drilling or boring them, which they perform for a trifle.

The inſtrument theſe people carry about with them for this purpoſe is of a very ſimple conſtruction, but requires much ſkill and exerciſe to uſe it; it is made in the following manner: The principal part conſiſts of a piece of ſoft wood, of an obtuſe, inverted, conical ſhape, about ſix inches high and four in diameter in its plain ſurface; this is ſupported by three wooden feet, each of which is more than a foot in length. Upon the upper flat part of this machine are holes or pits for the larger pearls, and the ſmaller ones are beat in with a wooden hammer. On the right ſide of this ſtool, half a cocoa-nut ſhell is faſtened, which is filled with water. The drilling inſtruments are iron ſpindles, of various ſizes, adapted to the different dimenſions of the pearls, which are turned round in a wooden head by a bow. The pearls being placed on the flat ſurface of the inverted cone, as already mentioned, the operator, ſitting on a mat, preſſes on the wooden head of his inſtrument with the left hand, while, with his right, he moves the bow which turns round the moveable part of the drill; at the ſame time he moiſtens the pearl, occaſionally dipping the little finger of the ſame hand into the water of the cocoa-nut ſhell with a dexterity that can only be attained by conſtant practice.

Amongſt the crowd are found vagabonds of every deſcription, ſuch as Pandarams, Andee or Hindu monks, fakirs, beggars, and the like, who are impertinently troubleſome. Two of theſe wretches particularly attracted the attention of the mob, though their ſuperſtitious penance muſt have diſguſted a man of the leaſt reflection: one had a gridiron, of one foot and a half long and the ſame in breadth, faſtened round his neck, with which he always walked about, nor did he take it off either when eating or ſleeping; the other had faſtened round that member which decency forbids me to mention, a braſs ring, and fixed to it was a chain, of a fathom in length, trailing on the ground; the links of this chain were as thick as a man's finger, and the whole was exhibited in a moſt ſcandalous manner.

The peſtilential ſmell occaſioned by the numbers of putre-

fying pearl fiſhes, renders the atmoſphere of Condatchey ſo inſufferably offenſive when the ſouth-weſt wind blows, that it ſenſibly affects the olfactory nerves of any one unaccuſtomed to ſuch cadaverous ſmells. This putrefaction generates immenſe numbers of worms, flies, muſkitoes, and other vermin, all together forming a ſcene ſtrongly diſpleaſing to the ſenſes.

Thoſe who are not provided with a ſufficient ſtock of money ſuffer great hardſhips, as not only all kinds of proviſions are very dear, but even every drop of good water muſt be paid for. Thoſe who drink the brackiſh water of this place are often attacked by ſickneſs. It may eaſily be conceived what an effect the extreme heat of the day, the cold of the night, the heavy dews, and the putrid ſmell, muſt have on weak conſtitutions. It is therefore no wonder, that of thoſe who fall ſick many die, and many more return home with fevers, fluxes, or other equally fatal diſorders.

The many diſappointments uſually experienced by the lower claſſes of men in particular, make them often repent of their coming here. They are often ruined, as they riſk all they are worth to purchaſe pearl ſhells: however, there are many inſtances of their making a fortune beyond all expectation. A particular circumſtance of this kind fell within my own obſervation: a day labourer bought three oyſters * for a copper fanam, (about the value of two-pence,) and was ſo fortunate as to find one of the largeſt pearls which the fiſhery produced this ſeaſon.

The donies appointed for the fiſhery are not all procured at Ceylon; many came from the coaſts of Coromandel and Malabar, each of which has its diſtinguiſhing number. About ten o'clock at night a gun is fired as a ſignal, when they ſail from Condatchey with an eaſterly or land wind, under the direction of a pilot. If the wind continues fair, they reach the bank before day, and begin diving at ſun-riſe, which they continue till the weſt or ſea-breeze ſets in, with which they return. The moment they appear in ſight, the

* The Eaſt India pearl ſhell is well known to be the *matrix perlarum* (mother-of-pearl) of Rumphius, or the *Mytilus margaritiferus* of Linnæus; conſequently, the general term pearl-oyſter muſt be erroneous: however, as it has long been in common uſe, I hope to be excuſed for continuing it.

colours

colours are hoiſted at the flag-ſtaff, and in the afternoon they come to an anchor; ſo that the owners of the boats are thereby enabled to get their cargoes out before night, which may amount to 30,000 oyſters, if the divers have been active and ſucceſsful.

Each boat carries twenty-one men and five heavy diving-ſtones, for the uſe of ten divers, who are called in Tamul *kooly kárer*; the reſt of the crew conſiſts of a tandel, or head boatman, and ten rowers, who aſſiſt in lifting up the divers and their ſhells.

The diving-ſtone is a piece of coarſe granite, a foot long, ſix inches thick, and of a pyramidical ſhape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers uſe another kind of ſtone, ſhaped like a half moon, to bind round their belly, ſo that their feet may be free. At preſent theſe are articles of trade at Condatchey. The moſt common, or pyramidical ſtone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally puniſhed or fined.

The diving, both at Ceylon and at Tutucorin, is not attended with ſo many difficulties as authors imagine. The divers, conſiſting of different caſts and religions, (though chiefly of Parrawer * and Muſſulmans,) neither make their bodies ſmooth with oil, nor do they ſtop their ears, mouths, or noſes with any thing, to prevent the entrance of ſalt water. They are ignorant of the utility of diving-bells, bladders, and double flexible pipes. According to the injunctions of the ſhark conjurer they uſe no food while at work, nor till they return on ſhore and have bathed themſelves in freſh water. Theſe Indians, accuſtomed to dive from their earlieſt infancy, fearleſsly deſcend to the bottom, in a depth of from five to ten fathoms, in ſearch of treaſures. By two cords a diving-ſtone and a net are connected with the boat. The diver, putting the toes of his right foot on the hair rope of the diving-ſtone, and thoſe of his left on the net, ſeizes the two cords with one hand, and, ſhutting his noſtrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl ſhells

* Fiſhermen of the Catholic religion.

as faſt as poſſible during the time he finds himſelf able to remain under water, which uſually is about two minutes. He then reſumes his former poſture, and making a ſignal, by pulling the cords, he is immediately lifted into the boat. On emerging from the ſea, he diſcharges a quantity of water from his mouth and noſe, and thoſe who have not been long enured to diving frequently diſcharge ſome blood; but this does not prevent them from diving again in their turn. When the firſt five divers come up, and are reſpiring, the other five are going down with the ſame ſtones. Each brings up about one hundred oyſters in his net, and, if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, inſtead of money, a fourth of the quantity which they bring on ſhore; but ſome are paid in caſh, according to agreement.

The moſt ſkilful divers come from Colliſh, on the coaſt of Malabar; ſome of them are ſo much exerciſed in the art, as to be able to perform it without the aſſiſtance of the uſual weight, and for a handſome reward will remain under water for the ſpace of ſeven minutes: this I ſaw performed by a Caffry boy, belonging to a citizen at Karical, who had often frequented the fiſheries of theſe banks. Though Dr. Halley deems this impoſſible, daily experience convinces us, that, by long practice, any man may bring himſelf to remain under water above a couple of minutes. How much the inhabitants of the South Sea Iſlands diſtinguiſh themſelves in diving we learn from ſeveral accounts; and who will not be ſurpriſed at the wonderful Sicilian diver Nicholas, ſurnamed the Fiſh *?

Every one of the divers, and even the moſt expert, entertain a great dread of the ſharks, and will not, on any account, deſcend until the conjurer has performed his ceremonies. This prejudice is ſo deeply rooted in their minds, that the government was obliged to keep two ſuch conjurers

* According to Kircher, he fell a victim among the Polypes in the gulph of Charybdis, on his plunging, for the ſecond time, in its dangerous whirlpool, both to ſatisfy the curioſity of his king, Frederic, and his inclination for wealth. I will not pretend to determine how far this account has been exaggerated.

always

always in their pay, to remove the fears of their divers. Thirteen of theſe men were now at the fiſhery from Ceylon and the coaſt, to profit by the ſuperſtitious folly of theſe deluded people. They are called in Tamul *Pillăl Kadtăr*, which ſignifies one who binds the ſharks, and prevents them from doing miſchief.

The manner of enchanting conſiſts in a number of prayers learned by heart, that nobody, probably not even the conjurer himſelf, underſtands, which he, ſtanding on the ſhore, continues muttering and grumbling from ſun-riſe until the boats return: during this period they are obliged to abſtain from food and ſleep, otherwiſe their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently ſo giddy as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats; which pleaſes them very much, as they have their protectors near at hand. Nevertheleſs, I was told, that in one of the preceding fiſheries a diver loſt his leg by a ſhark; and when the head conjurer was called to an account for the accident, he replied, that an old witch had juſt come from the coaſt, who, from envy and malice, had cauſed this diſaſter by a counter-conjuration, which made fruitleſs his ſkill, and of which he was informed too late: but he afterwards ſhewed his ſuperiority by enchanting the poor ſharks ſo effectually, that, though they appeared in the midſt of the divers, they were unable to open their mouths. During my ſtay at Condatchey, no accident of this kind happened. If a ſhark is ſeen, the divers immediately make a ſignal, which on perceiving, all the boats return inſtantly. A diver who trod upon a hammer-oyſter, and was ſomewhat wounded, thought he was bit by a ſhark, conſequently made the uſual ſignal; which cauſed many boats to return; for which miſtake he was afterwards puniſhed.

The owners of the boats * ſometimes ſell their oyſters, and at other times open them on their own account. In the latter caſe, ſome put them on mats in a ſquare, ſurrounded with a

* Theſe are the individuals which farm one or more boats from the renter; and though they are in poſſeſſion of them only during the fiſhery, they are commonly called the owners of the boats.

fence;

fence; others dig holes of almoſt a foot deep, and throw them in till the animal dies; after which they open the ſhells, and take out the pearls with more eaſe. Even theſe ſquares and holes are ſold by auction after the fiſhery is finiſhed, as pearls often remain there mixed with the ſand.

In ſpite of every care, tricks in picking out the pearls from the oyſters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpoſe: when a boat-owner employs a number of hired people to collect pearls, he places over them an inſpector of his own, in whom he can confide; theſe hirelings previouſly agree that one of them ſhall play the part of a thief, and bear the puniſhment, to give his comrades an opportunity of pilfering. If one of the gang happens to meet with a large pearl, he makes a ſign to his accomplice, who inſtantly conveys away one of ſmall value, purpoſely, in ſuch a manner as to attract notice. On this the inſpector and the reſt of the men take the pearl from him: he is then puniſhed, and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief ſecures the valuable pearl, and afterwards the booty is ſhared with him who ſuffered for them all. Beſides tricks like theſe, the boat-owners and purchaſers often loſe many of the beſt pearls while the dony is returning from the bank; for, as long as the animal is alive, and untouched, the ſhells are frequently open near an inch; and if any of them contain a large pearl, it is eaſily diſcovered, and taken out by means of a ſmall piece of ſtiff graſs, or bit of ſtick, without hurting the pearl fiſh. In this practice they are extremely expert. Some of them were diſcovered whilſt I was there, and received their due puniſhment.

Gmelin aſks, if the animal of the *mytilus margaritiferus* is an *aſcidia?* See Linn. Syſt. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately deſcribed; it does not reſemble the *aſcidia* of Linnæus, and may, perhaps, form a new genus. It is faſtened to the upper and lower ſhells by two white flat pieces of muſcular ſubſtance, which are called by Houttuin * *ears*, and extend

* Vide Houtt. Nat. Hiſt. Vol. I. p. xv. p. 381, ſeq.

about two inches from the thick part of the body, growing gradually thinner. The extremity of each ear lies looſe, and is ſurrounded by a double brown fringed line. Theſe lie almoſt the third of an inch from the outer part of the ſhell, and are continually moved by the animal. Next to theſe, above and below, are ſituated two other double fringed moveable ſubſtances, like the branchiæ of a fiſh. Theſe ears and fringes are joined to a cylindrical piece of fleſh of the ſize of a man's thumb, which is harder and of a more muſcular nature than the reſt of the body. It lies about the centre of the ſhells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fiſh which ſerves to open and ſhut the ſhells. Where this column is faſtened, we find on the fleſh deep impreſſions, and on the ſhell various nodes of round or oblong forms, like imperfect pearls. Between this part and the hinge (*cardo*) lies the principal body of the animal, ſeparated from the reſt, and ſhaped like a bag. The mouth is near the hinge of the ſhell, enveloped in a veil, and has a double flap or lip on each ſide; from thence we obſerve the throat (*œſophagus*) deſcending like a thread to the ſtomach. Cloſe to the mouth there is a curved browniſh tongue, half an inch in length, with an obtuſe point; on the concave ſide of this deſcends a furrow, which the animal opens and ſhuts, and probably uſes to convey food to its mouth *.' Near its middle are two blueiſh ſpots, which ſeem to be the eyes. In a pretty deep hole, near the baſe of the tongue, lies the beard (*byſſus*), faſtened by two fleſhy roots, and conſiſting of almoſt one hundred fibres, each an inch long, of a dark green colour, with a metallic luſtre;

* The depth at which the pearl fiſh generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the leaſt doubt it poſſeſſes, uſing for this purpoſe its tongue. This conjecture is ſtrengthened by the accurate obſervations made on muſcles by the celebrated Reaumur, in which he found that this body ſerves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fiſh, this changing of habitation has been long ſince obſerved by them. They allege, that it alters its abode, when diſturbed by an enemy, or in ſearch of food. In the former caſe they ſay it commonly deſcends from the ſummit of the bark to its declivity.

they

they are undivided, parallel, and flattened. In general, the *byſſus* is more than three quarters of an inch without the cleft (*rima*); but if the animal is diſturbed, it contracts it conſiderably. The top of each of theſe threads terminates in a circular gland or head, like the *ſtigma* of many plants. With this *byſſus* they faſten themſelves to rocks, corals, and other ſolid bodies; by it the young pearl fiſh cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleaſure. Small ſhell fiſh, on which they partly live, are often found clinging to the former. The ſtomach lies cloſe to the root of the beard, and has, on its lower ſide, a protracted obtuſe point. Above the ſtomach are two ſmall red bodies, like lungs; and from the ſtomach goes a long channel or gut, which takes a circuit round the muſcular column above mentioned, and ends in the anus, which lies oppoſite to the mouth, and is covered with a ſmall thin leaf, like a flap. Though the natives pretend to diſtinguiſh the ſexes by the appearance of the ſhell, I could not find any genitalia. The large flat ones they call males, and thoſe that are thick, concave, and vaulted, they call females, or *pedoo-chippy*; but, on a cloſe inſpection, I could not obſerve any viſible ſexual difference.

It is remarkable that ſome of theſe animals are as red as blood, and that the inſide of the ſhell has the ſame colour, with the uſual pearly luſtre; though my ſervants found a reddiſh pearl in an oyſter of this colour, yet ſuch an event is very rare. The divers attribute this redneſs to the ſickneſs of the pearl fiſh, though it is moſt probable that they had it from their firſt exiſtence. In the ſhade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower claſs of Indians, either freſh in their curries, or cured by drying; in which ſtate they are exported to the coaſt; though I do not think them by any means palatable.

Within a mother-of-pearl ſhell I found thirteen *murices nudati* (*vide* Chemnitz's New Syſtem, Cabt. Vol. XI. tab. 192, f. 1851 and 1852), the largeſt of which was three quarters of an inch long; but, as many of them were putrid, and the pearl fiſh itſelf dead, I could not aſcertain whether they had

crept in as enemies, or were drawn in by the animal itſelf. At any rate turtles and crabs are inimical to the animals, and a ſmall living crab was found in one of them.

The pearls are only in the ſofter part of the animal, and never in that firm muſcular column above mentioned. We find them in general near the earth, and on both ſides of the mouth. The natives entertain the ſame fooliſh opinion concerning the formation of the pearl which the ancients did: they ſuppoſe them formed from dew-drops in connection with ſun-beams. A Bráhmen informed me that it was recorded in one of his Sanſcrit books, that the pearls are formed in the month of May at the appearance of the Soatee ſtar (one of their twenty-ſeven conſtellations), when the oyſters come up to the ſurface of the water to catch the drops of rain. One of the moſt celebrated conchologiſts* ſuppoſes that the pearl is formed by the oyſter in order to defend itſelf from the attacks of the *pholades* and *boreworms*. But we may be aſſured that in this ſuppoſition he is miſtaken; for, although theſe animals often penetrate the outer layers of the pearl ſhell, and there occaſion hollow nodes, yet, on examination, it will be found that they are never able to pierce the firm layer with which the inſide of the ſhell is lined. How can the pearls be formed as a defence againſt exterior worms, when, even on ſhells that contain them, no worm-holes are to be ſeen? It is therefore more probable theſe worms take up their habitations in the nodes in order to protect themſelves from the attacks of an enemy, than that they are capable of preying on an animal ſo well defended as the pearl fiſh is. It is unneceſſary to repeat the various opinions and hypotheſes of other modern authors; it is much eaſier to criticiſe them, than to ſubſtitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the French Academy for 1712, is the moſt probable, *viz.* that the pearls are formed like bezoars and other ſtones in different animals, and are apparently the effects of a diſeaſe. In ſhort, it is very evident that the pearl is formed by an extravaſation of a glutinous juice, either within the body, or on the ſurface of the animal: the former caſe is the moſt com-

* The Rev. Mr. Chemnitz at Copenhagen.

mon. Between one and two hundred pearls have been found within one oyſter. Such extravaſations may be cauſed by heterogeneous bodies, ſuch as ſand, coming in with the food, which the animal, to prevent diſagreeable friction, covers with its glutinous matter, and which, as it is ſucceſſively ſecreted, forms many regular lamellæ in the manner of the coats of an onion, or like different ſtrata of bezoars, only much thinner; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be conſidered as the nucleus, or primary cauſe of its formation. The looſe pearls may originally have been produced within the body, and, on their increaſe, may have ſeparated and fallen into the cavity of the ſhell. Thoſe compact ones, fixed to the ſhells, ſeem to be produced by ſimilar extravaſation occaſioned by the friction of ſome roughneſs on the inſide of the ſhell. Theſe and the pearl-like nodes have a different aſpect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true, oval pearl, of a very clear water; while the node itſelf was of a dark blueiſh colour. The yellow or gold-coloured pearl, is the moſt eſteemed by the natives; ſome have a bright red luſtre; others are grey, or blackiſh, without any ſhining appearance, and of no value. Sometimes, when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one; but it oftener happens that, after having ſeparated the firſt coat, you find a worthleſs impure pearl. I tried ſeveral of them, taking one lamella off after another, and found clear and impure by turns; and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largeſt and moſt perfect pearl which I ſaw during my ſtay at Condatchey was about the ſize of a ſmall piſtol bullet, though I have been told ſince my departure many others of the ſame ſize have been found. The ſpotted and irregular ones are ſold cheap, and are chiefly uſed by the native phyſicians as an ingredient in their medicines.

We may judge with greater or leſſer probability, by the appearance of the pearl ſhell, whether they contain pearls or not. Thoſe that have a thick calcareous cruſt upon them,

to

to which *ſerpulæ* (ſea tubes) *Tubuli marini irregulariter intorti, Criſta-gali Chamar lazuras, Lepas tintinabulum, Madreporee, Millipore, Cellipore, Gorgontæ, Spongiæ,* and other Zoophytes are faſtened, have arrived at their full growth, and commonly contain the beſt pearls; but thoſe that appear ſmooth, contain either none, or ſmall ones only.

Were a naturaliſt to make an excurſion for a few months to Manâr, the ſmall iſland near Jafna, and the adjacent coaſt, he would diſcover many natural curioſities ſtill buried in obſcurity, or that have never been accurately deſcribed.

Indeed no place in the Eaſt Indies abounds more with rare ſhells than theſe; for there they remain undiſturbed, by being ſheltered from turbulent ſeas, and the fury of the ſurf. I will juſt name a few of them; viz. *Tellina foliaca* Linn *, *Tell Spenglerii, Arca culculata* †, *Arca Noæ, ſolen anatinus* Linn. *Oſtrea Iſognomum, Terebullum, albidum, ſtriatum, Turbo ſcalaris* ‡, *Bula volva* Linn. §, *Vexillum ingritarum, &c.* Amongſt the beautiful cone ſhells; *conus thalaſſiarchus Anglicanus cullatus* ||, *amadis thaſſiarchus con. generalcis* Linn. *c. capitaneus* ¶, *c. miles* **, *c. ſtercus muſcarum* ††, *c. reteaureum, c. glaucus* ‡‡, *c. cercola, regia corona, murus lapidius, cauda erminea, ſocietas cordium.* There are many others beſides thoſe already mentioned, equally valuable and curious.

The great ſucceſs of the Rev. Doctor John in conchology when at Tutucorin, and aſſiſted by G. Angelbeck, with a boat and divers; and the capital collections made by his agents, whom he afterwards ſent there with the neceſſary inſtructions and apparatus, may be ſeen in Chemnitz's elegant Cabinet of Shells in 4to, (with illuminated plates); and how many new ſpecies of Zoophytes he diſcovered, we learn from another German work by Eſper at Erlangen, the third volume of which is nearly finiſhed.

* The golden tong. † Mounkſcape. ‡ Royal ſtaircaſe.
§ Weaver's ſhuttle. || Red Engliſh admiral.
¶ Green ſtamper. ** Garter ſtamper.
†† Great ſand ſtamper. ‡‡ Capſ. Gottw.

VII. *Account of a new Method of Bleaching Cotton. By* C. CHAPTAL, *Member of the National Inſtitute* *.

THE happy applications which C. Berthollet has made of the oxygenated muriatic acid, in the bleaching of ſtuffs manufactured from vegetable ſubſtances, ſeems to have carried this art very near to perfection; but this method is not every where attended with the ſame degree of economy. Beſides, the proceſs requires very ſkilful hands, that the ſtuffs may not be deſtroyed by too corroſive leys, or leys improperly employed; and therefore we ought not to omit making known other proceſſes, in order that manufacturers may chooſe thoſe which they may think moſt beneficial. I ſhall therefore here deſcribe a proceſs, both ſimple and economical, for bleaching cotton yarn.

At the diſtance of about one foot four inches from the grate of a common furnace, place a copper kettle of a round form, one foot and a half in depth and four feet in diameter, and fix it in that poſition. The brim of this kettle, about ſix inches in breadth, being bent outwards, will reſt on the lateral edges of the maſon-work of the furnace. The remainder of the furnace muſt be conſtructed of cut ſtone, in the form of an oval boiler, ſix feet in height, and in breadth, meaſured from the centre, five feet. The upper part of the furnace has a round aperture, in diameter about a foot and a half. This aperture may be cloſed by means of a moveable ſtone, or a copper lid made for the purpoſe. On the edge of the copper kettle, which forms the bottom of this ſpecies of digeſter, place a kind of grate conſiſting of wooden bars, brought pretty near to each other in order that the cotton laid upon them may not fall through, and ſufficiently ſtrong to ſuſtain the weight of about 1600 pounds. When this apparatus has been conſtructed, impregnate the cotton diſpoſed in bundles, with a ſlight ſolution of ſoda, rendered cauſtic by lime: this operation muſt be performed in a wooden or ſtone trough, in which the cotton may be trod

* From the *Bulletin des Sciences*, Vol. II. No. 6.

upon

upon by ſecuring the feet with wooden ſhoes. When the alkaline liquor has well penetrated the cotton in an equal manner, it is to be carried to the boiler, and placed on the wooden grate already mentioned. The ſuperfluous liquor will run down between the bars, and form a liquid ſtratum, which will permit the maſs to be heated without any danger of burning the cotton or the bottom of the boiler. To form the alkaline ley, ſoda of Alicant may be employed equal to a tenth part of the weight of the cotton on which you operate; and in a boiler ſuch as I have deſcribed, about 800 pounds of cotton may be ſubjected to the proceſs at one time. At the moment when the cotton is introduced and arranged in the boiler, the aperture at top is ſhut, by its uſual covering, as cloſely as poſſible, in order that the vapours may aſſume a greater degree of heat, and re-act with more force on the cotton. The fire of the furnace is then to be kindled *, and the ley muſt be kept in a ſtate of ſlight ebullition for from twenty to thirty-ſix hours. It is then ſuffered to cool, and the cotton being carefully waſhed, muſt be expoſed on the graſs for two or three days, extending it on poles in the day-time, and ſpreading it out on the graſs during the night. The cotton will then have acquired a ſuperb degree of whiteneſs; and if, by chance, any parts of the cotton are ſtill coloured, it muſt be put into the kettle a ſecond time, or be expoſed a few days longer on the graſs. Theſe ſhades in the bleached cotton are owing in particular to its not having been all equally and completely impregnated with the ley; they may ariſe alſo from the arrangement of the cotton in the boiler, if it has been heaped up too much in certain points. When it is judged that the ley has been exhauſted by ebullition, the cover of the kettle is to be taken off, and the dry cotton muſt be beſprinkled with a new ſolution of ſoda; without this precaution it might run the danger of being burnt. It might readily be judged, by eſtimating the ſubſtances and time employed in this operation, that it is attended with economical advantages; but we have a more ſimple method of af-

* In giving the above dimenſions, I have ſuppoſed the fuel uſed to be coal: if wood is burnt, the dimenſions muſt be varied. In the latter caſe the bottom of the kettle would be too high above the bottom of the furnace.

certaining

certaining the fact, that is, the low price at which cotton is bleached in all thofe manufactories where this procefs is ufed. In the fouth of France, where this method is at prefent pretty generally adopted, eighty pounds of cotton are bleached for about feven fhillings fterling. This procefs was brought to us from the Levant fome time after the introduction of the procefs for dyeing the Adrianople red. It has been practifed for fome time, but kept a fecret till the prefent moment, and ftill known by the name of *blanchiment à la fumée*, bleaching by fmoke.

I do not know that this method has been applied to the bleaching of fpun flax or hemp. It would, however, be worth while to try the experiment: ftronger leys and longer ebullition, no doubt, muft be employed; but it is by experience alone that we can acquire information on the fubject.

VIII. *On the Method of preparing Inks that will withftand the Action of the Oxygenated Muriatic Acid. By* A. Bosse, *of Hamburgh* *.

INTRODUCTION.

FOR illuftrating the hiftory of this fubject, which has been fully treated of in the New Hanoverian Magazine, the following information may be neceffary:—Dr. Lentin, in his paper containing cautions in regard to the mifapplication of the oxygenated muriatic acid †, obferved, that with the help of this acid our common ink could be obliterated without the leaft injury to the paper which had been written with it, if the paper were firft drawn through diluted oxygenated muriatic acid; then through diluted fulphurous acid; and laftly, through water. At the fame time he added fome information which feemed to prove that this property had been employed in France feveral times to anfwer private purpofes, and to the prejudice of others. Soon after appeared a

* From *Scherer's Algemeines Journal der Chemie*, Vol. II. No. 10. The Introduction is by Dr. Scherer.

† Hannov. Mag. 1797, part 71.

paper by Philip Chriſtian Pitel of Minden *, in which he recommended an ink, diſcovered by him, which was indeſtructible, and could be obliterated neither by the oxygenated muriatic acid, nor by any other corroſive ſubſtance. This induced M. Wehrs to examine this ink, and the ſame thing was undertaken by M. Gruner †. According to their experiments, this ink, however, was obliterated in the courſe of nine hours by the oxygenated muriatic acid, and alſo by cauſtic pot-aſh; but at the ſame time an opinion entirely oppoſite was announced by Dr. Lentin ‡, M. Thorey §, and M. Wiegleb ||, who all, in conſequence of their having employed the ſame teſt, declared the ink to be indeſtructible. Theſe different opinions are therefore directly contrary to each other. M. Gruner found that the ink could be deſtroyed by the oxgenated muriatic acid, and by cauſtic alkali: M. Thorey obſerved, that its blackneſs was only leſſened by the oxygenated muriatic acid, and by the vapour of that acid. He found, however, as M. Gruner did, that the cauſtic alkali diſſolved the ink entirely from the paper, but that, like ſeveral acids, it deſtroyed the paper. M. Lentin and Wiegleb deduced from their experiments, thoſe of the former being made with oxygenated muriatic acid, and thoſe of the latter with the ſame, as well as ſeveral other acids, and even cauſtic alkali, that this ink was entirely indeſtructible. Theſe contradictory reſults M. Weſtrumb endeavoured to reſolve in his examination of Pitel's indeſtructible ink, which he found to be only common ink mixed with indigo; and he propoſed the following mixture for an ink which could not be deſtroyed:—Boil 1 oz. of Braſil wood, and 3 ozs. of pulveriſed galls, with 46 oz. of water; ſtrain the liquor, which muſt be boiled down to 32 ozs., and pour it, ſtill warm, over $1\frac{1}{2}$ oz. of perfectly pure ſulphate of iron, $1\frac{1}{4}$ oz. of gum arabic, and $\frac{1}{4}$ oz. refined ſugar. When theſe ingredients are diſſolved, add from 1 to $1\frac{1}{4}$ oz. good indigo ground exceedingly fine, and $\frac{3}{4}$ oz. of purified lamp black.

* Hannov. Mag. 1797, part 77. † Ibid. ‡ Ibid. p. 1223.
§ Ibid. 1797, part 97. || Reich's Anzeiger 1797, No. 297.

M. Bosse's

M. Bosse's Paper.

It is well known that M. Pitel, of Minden, gave the firſt account of a kind of ink which withſtood the oxygenated muriatic acid, at the deſire of Dr. Lentin, who excited the attention of the public to various deceptions practiſed at Paris with common ink by means of the oxygenated muriatic acid. As this ink conſiſted merely of a decoction of logwood and galls with water, in which ſulphate of iron, gum arabic, and ſugar had been diſſolved, and with which indigo and lamp black had been mixed mechanically, according to the analyſis of M. Weſtrumb, it may be eaſily explained why it exhibited ſuch different appearances when brought to the teſt. M. Gruner, apothecary in Hanover, as appears by his letter to the editor of the Hanoverian Magazine, was able to obliterate it entirely by the muriatic acid; while, on the other hand, its indeſtructibility by that acid is confirmed by M. Wiegleb and Thorey. Both are in the right: as ſoon as the ink is well ſtirred round, it withſtands this acid; but, if not ſtirred, this acid diſſolves the black oxyd of iron, and the paper in the places wrote upon is reſtored to its priſtine ſtate. Now, though this ink, when ſtirred round, does not entirely fail of its object, this circumſtance, however, may be often forgotten; and this the more readily, as even without ſtirring it has all the blackneſs when uſed of common ink, on account of the oxyd of iron which it contains. Since that time I have made experiments in order to produce an ink which might withſtand the oxy-muriatic acid, and yet contain no iron. I have been able to accompliſh this, as well as the compoſition of ſome other inks which contain iron, though it does not make their chief component parts, but ſerves only to give the ink a black colour. I made experiments alſo with the juice of green plants, according to the proceſs by which M. Murray made an indeſtructible kind of ink from the ſame ſubſtances. Iron muſt not be the chief component part of an ink deſtined to withſtand the muriatic acid, becauſe in the ſtate of a black oxyd it is eaſily diſſolved by the acid. But as it acts on vegetable colours only when they are capable of taking up oxygen, and does not deſtroy

them, but produces by its action a modification of the colour, they are exceedingly proper for this purpose. As none of these, however, were found entirely black, I was obliged to employ some mineral body which might be black and yet hold a great deal of oxygen. The most part of the metallic oxyds of a black colour contain very little oxygen, except that of manganese, which contains a great deal. I made choice therefore of this oxyd, and found, after repeated experiments, that it answered the intended purpose.

The oxy-muriatic acid dissolves all metals in a metallic form as well as metallic calces combined with a small quantity of oxygen, but not those which contain a great deal, as it has itself an excess of it. It is therefore capable,

1. Of dissolving metals in a metallic state, as the metals take up its superfluous oxygen, by which, as is well known, they become soluble in acids.

2. Such metallic oxyds as contain little oxygen are capable of abstracting it from acids, and they then dissolve in the acids, which then contain less oxygen.

The grounds which induced me to employ oxyd of manganese for preparing an ink capable of withstanding the oxy-muriatic acid, were as follows:—

1. Its black colour; 2. its containing a great quantity of oxygen, by which it is insoluble in the muriatic acid; 3. because it possesses so great an affinity for oxygen, that whenever it has lost any of its original quantity by being brought to a red heat, or exposed to the action of acids, it immediately draws it from the atmosphere, and again becomes black. I shall now describe the method of preparing this ink:—Boil 1 oz. of Brazil wood with 12 ozs. of water for a quarter of an hour; add $\frac{1}{2}$ oz. of alum: evaporate the whole to 8 ozs., and mix with the liquor 1 oz. of exceedingly soft, finely pulverised manganese, mixed up with $\frac{1}{2}$ oz. of pulverised gum arabic.

Brazil wood alone, by mere boiling, gives an ink not altogether unfit for use. Acids, less abundant in oxygen, such as the nitrous and muriatic acid, naturally exercise an action on this ink; but as these change the paper in a perceptible manner, deception is not to be apprehended.

Indigo also affords an ink that withstands the muriatic acid:

acid: it therefore makes a chief component part of that prepared by Pitel, but it is mixed in it only mechanically. The following is a preſcription for preparing an ink of the like kind, but in which the indigo is actually diſſolved.

Boil 1 oz. of Brazil wood, and 3 ozs. of coarſely pulveriſed galls, with 9 ozs. of vinegar and as much water, for the ſpace of eight minutes: in the liquor, after being ſtrained, diſſolve 1½ oz. of ſulphat of iron and 1 oz. of gum arabic, and then add to the whole a ſolution of ½ oz. of indigo in 1 oz. of concentrated ſulphuric acid.

The oxy-muriatic acid diſſolves the oxyd of iron in this ink, but the indigo remains undecompoſed. The principal point to be attended to is, that the ink may contain ſuch matters as are not affected by the oxy-muriatic acid: with other ſubſtances leſs deception is to be dreaded.

In the laſt place, I ſhall mention an ink made with the principal ingredients of common ink, but in preparing which, inſtead of the uſual liquids, I employed the expreſſed juice of ſome plant: the fitteſt for this purpoſe I found to be the leaves of the caper ſpurge, *Euphorbia lathyris* LINN.; the common holly, *Sambucus niger*, and common graſs.

IX. *Sketch of the Hiſtory of Mining in Devon and Cornwall. By Mr.* JOHN TAYLOR *jun. Miner, Taviſtock. Communicated by the Author.*

IT muſt be regretted that a ſubject of ſo great national importance, undertakings which engage ſo large a capital, and give employment to ſo many hands, as working the various mines in theſe kingdoms, ſhould gain but little attention from any but thoſe immediately concerned in them, and near the places in which they are ſituated; it is the more to be wiſhed that men of ſcience ſhould devote ſome thought to theſe concerns, as thoſe, under whoſe management moſt of theſe works are carried on, are often not men of ſufficient ability or ſcience to ſtrike out capital improvements. To be a good miner requires an active mind, with induſtry and ſtrict obſervation; theſe ſhould be accompanied by ſome general

general knowledge, at leaſt, of practical mineralogy, chemiſtry, mechanics, hydraulics, &c., and ſuch a knowledge of principles as might lead to improvements in the practical part of his buſineſs. It would be unreaſonable to expect to find theſe qualifications general at preſent, but the exiſting ignorance might ſurely be ſoon removed, if, the attention of men of ſcience being turned to the ſubject, they would take pains to point out to the practical miner the improvements of which his operations are ſuſceptible.

If it be true, as was aſſerted in the number of the Philoſophical Magazine for February laſt, "that the pooreſt mine in Cornwall is worked at a greater expence than the richeſt one on the Continent," much is to be done; although to work a mine rich or poor to the ſame extent muſt require the ſame expence. From my own experience I cannot aſſent to what is aſſerted, in the ſame paragraph, of the captains or directors of the Corniſh mines; I can bear teſtimony to there being among them men of ability, obſervation, and liberality; of all the preſent practical knowledge of the ſubject many of them are perfect maſters, and have not often, I think, that deſire of excluding interference which is imputed to them.

Mining is a ſubject requiring ſtudy, and deſerving the attention of the philoſopher, as it tends to unveil ſome of the moſt hidden proceſſes of Nature, and to anſwer important ends to Society. On the Continent many of the moſt eminent men have not thought it beneath them to undertake the management of mines; and to this it is fair to impute the capital manner in which ſome foreign mines are reported to be conducted.

It muſt afford pleaſure to know that of late years many improvements in mining, as well as in moſt other arts connected with or depending on the ſciences, have taken place; and both the diſcovery of the ores, and their treatment after being brought to the ſurface, is, in moſt reſpects, carried on with greater certainty and ſkill, and conſequently more profit.

It may afford ſome amuſement to attend a little to the hiſtory of this ſource of the moſt ancient traffic of our iſland, and the cauſe, probably, of much of our civilization, it having attracted,

attracted, long ago, the viſits of other more enlightened nations.

The tin of Britain was known in diſtant parts of the world at a very remote period. It is generally believed that the Phœnicians were the nation principally engaged in trading with Britain for this article. Tin works were certainly carried on before iron was in uſe in England. Many tools of oak are now found which tradition among the tinners make to have belonged to the Saxons or Danes; but it is probable, for the reaſon before ſtated, that they were employed before the time of their having a footing in this country.

The greateſt quantity of tin was formerly, it is aſſerted, found on the foreſt of Dartmoor, in the county of Devon; and works to a vaſt extent muſt have been carried on there, if it be true, as we are told, that thirty thouſand men found employment upon this tract; for all the miners now in Cornwall do not amount to more than ten thouſand. It is, however, to be conſidered, that in the ancient mode of working, without the aid of machinery, many more hands muſt have been neceſſary than at preſent—I ſhould think more than in the proportion of three to one. All theſe works on Dartmoor were only on or near the ſurface: now, however, the lodes * or veins of tin found on this diſtrict are not valuable enough, or do not continue to ſuch a depth as to make them very profitable to work.

Almoſt all the tin procured in former ages was probably from ſtream-works, in bottoms or low grounds, where fragments of the ore, waſhed from the lodes in the neighbouring hills, ſubſide, and are ſeparated from the earth in a granulated form by waſhing. This, of courſe, is obtained without any ſubterranean work. In ſuch ſituations as theſe probably metals were firſt diſcovered, mixed merely with the upper ſoil, or lodging in clefts of rocks. Thus is gold found in America, in Africa, in the county of Wicklow in Ireland, and ſo has it been occaſionally diſcovered in Cornwall; and veins or lodes of this metal might doubtleſs be found near the places where ſuch depoſitions are met with.

The tracing and following lodes of ores into the earth is a

* The name given in Cornwall to veins in which ore is found.

more difficult taſk than coming at the metal in the way of ſtream-works; it requires more energy of mind, and a more advanced ſtate of the arts. The difficulty that muſt attend keeping a mine, ſunk to ſome depth, free from water by manual labour only, could not but prevent men, before the application of machinery, from diving deep into the bowels of the earth. We have, however, ſome inſtances where old workings are found at ſo great a depth as to be even now with difficulty kept dry by means of machinery; but theſe, though they may be counted ancient, were probably opened long ſubſequent to the origin of mining in this county.

There are many mines which could not poſſibly be worked without the aid of gunpowder, and, until the diſcovery of this powerful agent, underground operations muſt have been uncertain and difficult. The hammer and wedges of metal were probably the firſt inſtruments for ſplitting rocks (and they ſtill continue, in the ground that will yield to them, to be much uſed in Cornwall), and the pick, or inſtrument for cleaving the ground, having a head for driving the wedges called by the miners *gads*, from a Corniſh word *gedn*, a wedge. The form of theſe inſtruments found in old works, I think, offers an evidence of their antiquity. A pick, which was found in Wheal * Unity tin mine, in Devon, in a part not worked certainly for more than eighty years, and which could not probably be reckoned to be leſs than 100 years old, does not differ materially from the form of that now uſed; while one diſcovered in old workings in Drake Walls mine, in the pariſh of Calſtock in Cornwall, about ten miles diſtant from the other mine, is of ſo different a ſhape as to make one conclude, judging by the ſlow progreſſion of changes effected on common inſtruments, that it is of much higher antiquity. Wedges of dry wood were alſo very ingeniouſly made uſe of, by driving them into the clefts, and then wetting them, ſo as to cauſe them to ſwell and force the ground aſunder. Fire was an agent long ago employed for ſplitting the rocks; but the effects of gunpowder ſo far exceed any thing before made uſe of for ſuch purpoſes, that its diſ-

* In this part of the country the word *wheal*, ſignifying in the Corniſh language *a work*, is generally prefixed to the proper names of mines.

covery

covery and application to works of this kind form a grand epoch in the hiſtory of mining. This, it appears, took place in Hungary or Germany about the year 1620, and was firſt introduced into England at the copper mine at Ecton, in Staffordſhire, about the year 1670, by German miners, brought over by Prince Rupert; but it was not in uſe in Somerſetſhire till 1684, after which, probably, the Corniſh miners became acquainted with this powerful aſſiſtant to their operations.

Tin was the firſt object of the Corniſh miner's ſearch, and I ſhall therefore firſt take notice of the hiſtory of the tin-works. It was probably firſt found, as I have obſerved, near the ſurface, and not in regular veins; almoſt all the low grounds in Cornwall, and I believe every bottom on Dartmoor foreſt in Devon, bear the marks of having been *ſtreamed*. The Romans, probably, intereſted themſelves in the working of the mines; indeed, one principal inducement to that people's turning their attention to this iſland, ſeems to have been the metals that were reported to them to be found here. The Saxons neglected theſe hidden treaſures, but the Normans worked them to great advantage. From that time to the end of the reign of John the mines were not profitable, and moſtly in the hands of the Jews. They revived in the time of his ſon Richard; but in the reign of Edward I. the Jews were baniſhed the kingdom, and the mines neglected. Edmund, the eldeſt ſon of the king, and earl of Cornwall, however, made ſome important alterations in the regulations of the tin-works by a charter, which was confirmed by Edward I. in the latter part of his reign. Indeed it is from this time that the peculiar laws and privileges relating to the Stannaries are chiefly to be dated. Mining infringed in ſome inſtances on property, and cauſed diſputes, beſides requiring indulgences not general; and thus caſes aroſe not cogniſable by the common law. In this way a peculiar code, ſpringing from cuſtom, took its riſe; and though this in ſome meaſure exiſted before, yet it was not till this period that it was confirmed by royal charter, and enforced by ſubſequent acts of parliament. It was by this charter that the bounding land to the purpoſe of tinners working on it, the duties to

the earl of Cornwall, and the coinages of tin, or ſtamping with the earl's ſeal, were firſt eſtabliſhed. Before the reign of Edward I. tinners worked in the earl's land only, paying him a fifteenth part of what they got, and they were not at all permitted to dig in ſanctuary ground, churches, mills, houſes, gardens, and ſo on; and if in working under they chanced to ſubvert any houſe, or to damage a high-way, they were obliged to make it good. When it became an object to ſearch throughout any place or perſon's lands, a court alſo to determine caſes relating to the tin-works became neceſſary; and this, adjudging under the authority of, and according to the code of laws before mentioned, was firſt eſtabliſhed by Edward I. and is called the Stannary Court.

In Cornwall and Devon two different ſyſtems of Stannary laws now exiſt: thoſe in Cornwall have been from time to time amended and corrected; but in Devon, where mining has for a long while ſlumbered, the laws continue in their original crude ſtate. As the ſpirit, however, of ſearching into the bowels of the earth ſeems again to be reviving in this county, the laws, ſhould they be called into action, will probably undergo ſome reviſion.

The duke of Cornwall had royal juriſdiction, and when the duchy came into the hands of the crown, which happened in the reign of Edward III. he made his eldeſt ſon, the Black Prince, the duke, and appointed it, with the revenue ariſing from it, to the ſons and heirs apparent of the crown, though no ſon of the king can be duke of Cornwall but the firſt-born, even though heir apparent.

The Stannary laws were explained and confirmed, with ſome alterations at different times, till Arthur, eldeſt ſon of Henry VIII. made certain conſtitutions relating to the Stannaries, which the tinners refuſed to obſerve: the king, after prince Arthur's death, ſeized the charter as forfeited, but granted a new one with freſh privileges, appointing all new laws relating to the tinners to be made by a parliament of their own body; and upon this footing the Stannaries remain.

Great powers have been at times aſſumed by this juriſdiction,

tion, it has even taken upon it the trial for felony in the caſe of a tinner; in general, however, the caſes where its authority interferes, are in diſputes concerning tin-works or between tinners: it provides a ſupply of water to a tin-work, giving a power to conduct it through any lands for that purpoſe.

One of the improvements, with regard to tin particularly, was the invention of ſmelting in reverberatory furnaces with pitcoal, inſtead of the old *blowing-houſes*, as they were called, with charcoal fires. When this change was firſt introduced is not perhaps certainly known: Dr. Watſon, in his Chemical Eſſays, ſtates, that Becher reſided in Cornwall ſome years before he died, which was in the year 1682; and that he made many improvements in the working of mines and fluxing of metals, and introduced there the method of ſmelting tin by the flame of pitcoal thrown upon the ore in a reverberatory furnace, inſtead of the fire of wood or charcoal, in the way formerly made uſe of. He takes notice of this in the Dedication to Mr. Boyle of his *Alphabethum Minerale*, written at Truro in 1682, not long before he died. Pryce, in his *Mineralogia Cornubienſis*, ſtates, that this mode was firſt prompted by neceſſity, and many experiments were made upon it by Sir Bevil Granville, of Stow, in the time of Charles I. though it was not effectually done till the reign of Queen Anne.

I find, in the reviſion of the Stannary laws at the convocation of the parliament of Stannators aſſembled in the 26th year of the reign of George II. mention is made of the "ancient laws and conſtitutions of the Stannaries relating to tin-blowers, and refining of tin in *blowing-houſes*, which was formerly the only method of refining of tin;" and it is ſaid, "of late years there has been another method found out of refining of tin by means of reverberatory furnaces, which is commonly called ſmelting of tin."

Copper, of which ſo much is now ſent from this part of the country, was not an object attended to, till a comparatively late date, by the Corniſh miners: even in tin mines, which as they deepened produced copper, as is often the caſe, and where they needed to raiſe this ore, it was thrown by as of no value, going by the name of *poder*. Thoſe who

live in the preſent more enlightened period, are now reaping profits left to them by the ignorance of their forefathers. This proves of what conſequence it is to determine, if poſſible, the value of all the ſubſtances paſſing under the miner's obſervation.

Copper, however, was probably worked at a remote period in Wales, at the Parys mountain, which indeed is ſuppoſed to derive its name from the Celtic word *praas*, braſs or precious metal; and this would offer a proof of its having been worked by the ancient Britons. It was not attended to, or at leaſt not well underſtood, in England, till the reign of Queen Elizabeth, who paid great attention to the mines of the kingdom, and, by granting great privileges to Daniel Houghſetter, Chriſtopher Schutz, and other Germans whom ſhe invited into England, commenced and eſtabliſhed the highly valuable and important buſineſs of finding and purifying this uſeful metal. To theſe foreigners, too, is owing the flouriſhing ſtate of our braſs manufactories. In this reign Parys mine was granted to patentees, but was not worked, at leaſt to any advantage, for a century and a half.

The copper mines of Cornwall now are works of great magnitude, and ſome have been ſunk to an amazing depth, and are kept working at a vaſt expence. The quantity of materials conſtantly uſed forms an extenſive commerce to this part of the kingdom; and this, as well as the mines themſelves, gives employment to numbers.

The other metals are not found in Cornwall in great abundance. Lead, with ſilver contained in the ore, is found in ſome places; and in Devonſhire, on the borders of this county, a very large mine at Bere afforded galena very rich in ſilver. It was formerly worked as a royal mine, but afterwards lay idle a long time. It has ſince been feebly tried; but though a ſteam-engine was on it, they could not go ſo deep as it had been before ſunk without more power to draw the water, and not finding much valuable return, the mine has been lately ſtopped again. Veins of lead ore ſometimes run with copper lodes.

Iron is found in ſome places, but not in large quantities; and coals being diſtant, it will not repay the coſt of procuring.

The

The ores of many of the ſemi-metals, as zinc, cobalt, arſenic, manganeſe, &c. are ſcattered in different places, and of late have received attention.

Mining has been much improved within this century. Till about a hundred years ago, the water was drawn from the mine by dint of human labour; but within theſe ſeventy years the application of hydraulic engines has become general. The introduction of the ſteam-engine formed a grand æra in the annals of mining; as in many ſituations no other power, that could be commanded, could poſſibly work the pumps neceſſary to raiſe the water from the bottoms: the conſequence has been, that many mines have been worked that were formerly abandoned as impoſſible to be proſecuted. Other improvements of late years have been creeping in; the dreſſing of ores, or properly ſeparating them from the earths, &c. in which they lie, is much better managed than formerly.

Great ſpirit is to be ſeen in the works in Cornwall; prodigious ſums are laid out in erecting powerful machinery, ſometimes even before any quantity of ore is ſeen. This often well repays thoſe who expend it, eſpecially if done under the direction of experienced and ſkilful managers, who are good judges of the appearances and ſymptoms on which is founded the expectation of finding a valuable return.

The very ſpirited and laudable exertions that are making, at a great riſk, to bring to the public uſe ſo valuable an article as the copper mines produce, have lately been damped by ſhutting up the channel of trade with regard to this commodity. The purpoſe for which this is done, namely, a reduction in the price, cannot in the end be anſwered; for even ſhould that for a time take place, the deep, and conſequently expenſive mines, and thoſe which produce ores of inferior quality, will probably be ſtopped; and thus, a ſmaller quantity coming to market, the price muſt again advance, perhaps higher than it now is. In the mean time, ſhould it operate to reduce the price, a valuable ſource of traffic will be loſt to the nation; the revenue will ſuffer; individuals be diſtreſſed; and numbers of people not uſed to any other way of life become deſtitute of the means of exiſtence.

X. *On*

X. *On the relative Proportions of Coals and Iron-Stones uſed at the Blaſt-Furnace, and of their proper Application to Uſe. By Mr.* DAVID MUSHET, *of the Clyde Iron-Works. Communicated by the Author.*

IN the ſmelting operation a juſt proportion and aſſociation of materials and mechanical conſtruction ought to be blended in order to produce the beſt poſſible effects. Under the former are comprehended the cokes, iron-ſtone, limeſtone, and blaſt; by the latter is underſtood the furnace, the power of the blowing-machine, or the compreſſion and velocity under which the air is diſcharged into the furnace, and the genius or mechanical ſkill of the workmen. According to this diviſion I ſhall endeavour to point out the very various effects which diſproportion in any caſe produces, and *vice verſa*.

In the preceding papers the coal and iron-ſtone have been traced through their various ſtages of preparation, and that ſtage pointed out in which they were moſt ſuitable for the profitable manufacture of the metal. It will be neceſſary to carry along with us this fact, that in the exact proportion which the quantity of carbon bears to the quantity of metal in the ore, and its mixtures, ſo will be the fuſibility, and of courſe the value of the pig-iron obtained. The importance of this truth will ſtill farther appear when we conſider the very various qualities of pit-coal, the different proportions of carbon which they contain, and the various properties attached to every ſpecies of this uſeful combuſtible.

Among the many ſtrata of coal which I have diſtilled, ſome I have found to contain 70 parts in the 100. This large proportion is peculiar to the clod-coal, uſed at ſome of the iron-works in England, and juſtly preferred, for the purpoſe of manufacture, to the pureſt and hardeſt variety of ſplint-coal. The latter I have found to average from 50 to 59 parts of carbon in the 100; and the ſoft, or mixed qualities of coal, from 45 to 53 parts. Such various proportions of carbon plainly point out, that the operations to be followed at each individual iron-work ought not to reſt upon

precedent, unleſs borrowed from thoſe works where exactly the ſame quality of coal is uſed. This analyſis alſo lays open part of the ſource from whence originates the widely different quantities of metal produced *per* week at various blaſt-furnaces, and the great diſproportions of ore uſed to different coals.

Experience has ſhewn that the three qualities of coal juſt mentioned, will ſmelt and give carbonation to the following proportions of the ſame ſpecies of torrefied iron-ſtone:—

112 lb. of clod-coal cokes will ſmelt - - 130 lb.
112 lb. of ſplint-coal cokes will ſmelt - - 105 lb.
112 lb. mixed ſoft and hard coal cokes will ſmelt 84 lb.

Let the iron-ſtone be ſuppoſed in the blaſt-furnace to yield 40 *per cent.* then we find that the 1-20th of a ton of the reſpective qualities of cokes will ſmelt and carbonate the following proportions of iron, *viz.*—112 lb. clod-coal cokes, 130 lb. iron-ſtone, at 40 *per cent.* = 52 lb. iron; 112 lb. of ſplint-coal cokes, 105 lb. of the ſtone = 42 lb. of iron; and 112 lb. ſoft and hard coal cokes, 84 lb. of the iron-ſtone = 33 $\frac{6}{10}$ lb. of iron. We then have for the quantity of metal produced by one ton of each quality of cokes:

Clod-coal 52 × 20 = 1040 lb.
Splint ditto 42 × 20 = 840 lb.
Mixed ditto 33 $\frac{6}{10}$ × 20 = 702 lb.

This furniſhes a datum whereby we eaſily obtain the quantity of the various cokes neceſſary to produce 1 ton of carbonated crude iron by common proportion: for if 1040 lb. of metal are produced by 1 ton, or 2240 lb. of clod-coal cokes, the quantity of the ſame cokes requiſite for the production of 1 ton, or 2240 lb. of metal, will be:

		T.	*C.*	*Q.*	*lb.*
	4824·6 lb. =	2	3	0	8
Splint-coal cokes 840 : 2240 :: 2240 : 5973·3 lb. =		2	13	1	9
Mixed ditto 702 : 2240 :: 2240 : 7147·5 lb. =		3	3	3	7

If to the quantity of cokes neceſſary to manufacture 1 ton of crude iron, we add the quantity of volatile matter driven off in the proceſs of charring, which may be thus eſtimated upon the average of each quality:—

Clod coal $\frac{3}{5}$ or 37 $\frac{1}{2}$ *per c.* produce in cokes $\frac{5}{8}$ *a* 62 $\frac{1}{2}$ *per c.*
Splint coal $\frac{4}{5}$ — 50. —— $\frac{4}{8}$ or 50
Mixed coal $\frac{5}{8}$ — 62,5 —— $\frac{3}{8}$ — 37 $\frac{1}{2}$

Then,

Then, for the quantity of the reſpective coals uſed in the raw ſtate, we have the following reſults in proportion:—

				T.	C.	Q.	lb.
Clod-coal	5 : 4824·6 :: 8 :	7719$\frac{3}{4}$	=	3	8	2	19
Splint-coal	4 : 5973·3 :: 8 :	11946	=	5	6	2	18
Mixed coal	3 : 7147·1 :: 8 :	19158$\frac{2}{3}$	=	8	11	0	16

Theſe great diſproportions of quantity, uſed to fabricate 1 ton, or 2240 averdupoiſe pounds of the ſame quality of crude iron, will convey a ſtriking and impreſſive idea of the multifarious qualities of coal which may be applied and made to produce the ſame effects. It ſhould alſo convince the manufacturer that the ſtudy and analyſis of his own materials is the firſt and radical approach to true knowledge, and certainty of operation. Diveſt him of this knowledge, and view him guided by the *cuſtoms* and *rules* prevalent at another manufactory, where the coals and ores may be as different as has been already mentioned, and we will no longer wonder at the uncertainty of his reſults, and the numberleſs errors of his direction.

Before I enter into the practical diſcuſſion of the application of coal, I beg leave to indulge myſelf in the following calculations:—We have already ſeen that the production of 2240 lb. of carbonated crude iron requires 4824 lb. of clod-coal cokes; theſe may be averaged to contain 4·5 *per cent.* of aſhes, which, deducted from 4824, gives 4607 lb. of carbon uſed for 1 ton of metal: this ſum, divided by 2240, farther gives, for 1 lb. of caſt iron thus manufactured, 2·056 lb. of carbon.

We next find that 2240 lb. of the ſame metal requires of ſplint-coal cokes 5973·3 lb.; we farther find, from a table of the analyſis of coal, furniſhed in a former paper, that 100 parts of the raw coal contained 4·2 parts of aſhes. As it is there ſtated to loſe 50 *per cent.* in charring, 100 parts of cokes will contain 8·4 of aſhes; and 8·4 *per cent.* deducted from 5973·3, gives 5472 lb. of carbon. This again, reduced by 2240 lb. gives for each lb. of metal manufactured, 2·442 lb.

Again, 7147·1 lb. of cokes produced from ſoft mixed coals are conſumed for every ton of 2240 averdupoiſe pounds of crude iron produced; every 100 parts of the ſame coals contain

tain 3·3 parts of aſhes; and 100 parts of cokes contain nearly 6·5 *per cent.* of aſhes, which, deducted from 7147·3, gives 6672·6 of carbon, which divided by 2240, gives, for the quantity uſed for 1 lb. of caſt iron, 2·978 lb.

From theſe calculations it appears, that 2240 lb. of carbonated iron, requires of carbon from clod-coal 4607 lb.; of carbon from ſplint-coal, 5472 lb.; and of carbon from mixed coal, 6672 lb.: that 1 lb. of carbonated iron requires of carbon from clod-coal cokes 2,056 lb.; from ſplint, 2,442 lb.; from mixed, 2,983 lb.: and that carbonated crude iron may be obtained when widely different quantities of carbon have been conſumed.

In ſeeking for a ſolution of the latter fact, we muſt have recourſe to the different degrees of inflammability of the carbon, according to the various laws of continuity impoſed upon it in its foſſil conſtruction. It can eaſily be conceived, that, owing to this ſtructure, and the nature of the interpoſed aſhes, the particles of carbon of ſome cokes will be more eaſily oxygenated than thoſe of others; in the ſame way that we find ſplint-coal, when expoſed to ignition in contact with open air, affords 1-3d of more cokes than are obtained from ſoft mixed coals, though the latter, when diſtilled, yields more pure carbon than the former.

By experiment it is proven, that 100 grains of carbonic acid gas is compoſed of 72 parts of oxygen united with 28 parts of carbon: if the quantity of the carbon of clod-coal, *viz.* 2·056 lb. uſed for the manufacturing of every pound of caſt iron, is reduced to grains, we will find it to conſiſt of 14392 grains; this, divided by 28, gives the acidifiable principle of $514 \times 100 = 51400$ grains of carbonic acid gas *:

* This is ſuppoſing, for the moment, that the whole of the carbon is oxygenated, either by the oxygen contained in the ore, or obtained from the diſcharging-pipe by the decompoſition of the atmoſpheric air: this, however, is not ſtrictly true, as the metal takes up a ſmall portion, by weight, of the carbon; and when, by accident, moiſture has been introduced into the furnace, either through the medium of the blaſt, or of the materials, its decompoſition furniſhes a portion of both oxygen and hydrogen, which may diſſolve, and alſo carry off, a part of the carbon. Atmoſpheric air being found to hold water in ſolution, a ſmall quantity of hydrogen will, even in the dried weather, be preſent in the blaſt-furnace. M.

hence, as 1 cubic foot of this gas, at 29·84 of barometrical preſſure, and 54·5 of temperature, weighs nearly 761 grains, we find, that in the formation of every pound of caſt iron $\frac{51400}{761} = 67{,}54$ cubical feet of carbonic acid gas will be formed; and in the production of 1 ton of metal, the aſtoniſhing quantity of 151289,60 cubic feet. This quantity, however incredible it may ſeem, is only what would be formed under the above preſſure, and at the above temperature: when we take into the account the high temperature at which the decompoſition and recombination are effected, with the conſequent increaſe of claſtic force and of volume, our ideas are almoſt unable to commenſurate the ſum of the gas hourly formed, and thrown off, ignited to the higheſt degree of heat.

If the ſame mode of calculation is adopted with the other qualities of coal, we will have the following reſults:—

For the ſplint-coal 2,442 lb. or $\frac{17094}{28} = 610{,}5 \times 100$ = 61050 grains of carbonic acid, which gives $\frac{61050}{761} =$ 82,85 cub. feet for 1 lb., and 82,85 × 2240 = 185,584 cub. feet for 1 ton. For the mixed coal 2,983 or $\frac{20881}{28} = 710 \times$ 100 = 71000 grains carbonic acid; that is, $\frac{71000}{761} = 93{,}3$ cubical feet for 1 lb.; and 93,3 × 2240 = 208,992 cubical feet for 1 ton. By the ſame calculation we may attain a pretty accurate notion of the quantity of atmoſpheric air neceſſary to produce 1 lb. or 1 ton of caſt iron; an average of the three varieties of coal will be ſufficiently accurate for this this purpoſe; thus $\frac{14392 + 17094 + 20881}{3} = 17455\frac{1}{3}$ or 2,4935 lb. of carbon are conſumed upon the average of each pound of pig-iron: this is found to produce of carbonic acid gas $\frac{17455\frac{2}{3}}{28} = 62{\cdot}341 \times 100 = 62{\cdot}30041$ grains; which again divided by 761, the grains in one cubic foot gives 81·86 cubic feet for the gas diſcharged in manufacturing one pound

of caſt iron. As carbonic acid contains, as has already been noticed, 72 parts of oxygen in 100, then we have for the quantity of oxygen gas 100 : 72 :: 62300·41 : 44856·29 grains oxygen gas; and as, at the ordinary temperature and preſſure of the atmoſphere, a cubic foot of oxygen gas weighs 591 grains, we find 44856·29 divided by 591 = 75·89 cubic feet of oxygen gas neceſſary to form the acidifying principle of 81·86 cubic feet of carbonic acid gas; and that the ſame quantity of oxygen gas is neceſſary to the production of one pound of carbonated crude iron. This leads us to the following ſtatement for the quantity of atmoſpheric air uſed during the ſame operation; firſt premiſing that the conſtituent parts of atmoſpheric air are nearly 73 of azote and 27 of oxygen gas; of atmoſpheric air then neceſſary, we have 27 : 100 :: 75·89 : 281 cubic feet.

I ſhall now proceed from mere calculation to matter of fact, and attempt to prove the correctneſs of the former by the approximation of the latter to its reſults. Let a blaſt-furnace be ſuppoſed to produce 20½ tons of pig-iron *per* week, = 45360 averdupoiſe pounds; this, divided by days, hours, minutes, and ſeconds, gives *per* day 6480 pounds, *per* hour 270, *per* minute 4½ lb., and *per* ſecond 525 grains.

From this it is evident that 1 lb. of caſt iron is produced in $13\frac{33}{50}$ ſeconds: experience has ſhewn that a blaſt-furnace, producing, in any of the above periods, the reſpective quantity of metal, requires a diſcharge of air *per* minute nearly equal to 1350 cubic feet; this, divided by 4,5 lb., the quantity produced *per* minute gives, for 1 lb. iron, 300 cubic feet. The quantity, by calculation, we have ſeen to be 281 cubic feet—difference 19: a ſum no way conſiderable when we reflect upon the inequality of the movements of a blowing machine, and when it is recollected that ſome allowance ought alſo to be made for what air may paſs through the furnace undecompoſed, or may be loſt at the place of entrance.

From this coincidence of theory with practice, we cannot help admiring the rigorous principles on which the Lavoiſerian ſyſtem is founded; nor are we leſs pleaſed to find, that, ſmall as the operations of the chemiſt may be, yet they are a

juſt epitome of what takes place in the philoſophy of extenſive manufactories. The following table exhibits the quantity of carbon which may be uſed upon an average, with the relative quantity of carbonic acid formed, and air uſed:—

In the manufacture of	1 lb.	—	1 ton of iron,
The pure carbon requiſite is	2·49	—	5585·44 lb.
Carbonic acid formed -	81·86	—	183366·40 cub. feet
Oxygen gas uſed - -	75·89	—	169993·60 cub. feet
Atmoſpheric air employed	281·00	—	629440·00 cub. feet.

From the foregoing particulars upon coal may be learned how much is dependent upon the native conſtruction of coal and its conſtituent parts; I ſhall next advert to the effects produced by its improper preparation.

When coals intended for the blaſt-furnace are ſufficiently charred, they ought, in point of colour, to be of a ſilver grey; their fracture will appear lamellated and porous if ſplint-coals have been uſed; ſofter coals form themſelves into branches ſlightly curved, and, when properly prepared, are always very porous. I have frequently found that the better the cokes were charred, the more water they will abſorb. Coals half burnt do not take up half ſo much water, becauſe their fracture continues in part to be ſmooth and leſs porous than when thoroughly burnt.

When half-prepared cokes are introduced into the furnace, the metal formerly carbonated will loſe its grey fracture, and approach to the quality of oxygenated iron. Their preſence is eaſily detected by the unuſual quantity of thick vapour ariſing along with the flame. Beſides, the water and ſulphur, which raw coals introduce into the furnace, and which always impair the quantity of carbon by the various ſolutions effected by the preſence of oxygen, hydrogen, &c. the fitneſs of the coal for combuſtion, and the ſupport of the ore, is much diminiſhed by this ſecond courſe of ignition and diſengagement of bitumen. The preſſure of the incumbent ores alſo fracture, and reduce the cokes into ſmall pieces, which produce a conſiderable portion of coke-duſt; this is partly carried to the top of the furnace before the blaſt: ſometimes below it appears in immenſe quantities, ignited to whiteneſs, and liquid as ſand. Coal thus detached from the maſs, expoſed

poſed to the action of a compreſſed current of air, is unfit for conveying the carbonic principle to the metal; and as it frequently belongs to the juſt proportion of charcoal neceſſary to ſmelt the ores, and to carbonate their iron, its loſs muſt be felt, and the quality of iron impaired.

When cokes of any quality are expoſed to a moiſt atmoſpheric, ſo as to abſorb water, their effects in the blaſt-furnace become much reduced, and the preſence of the water is productive of the moſt hurtful conſequences in the production of carbonated crude iron. I have found, by repeated experiment, that 1 lb. of well-prepared cokes will, when laid in water, take up $1\frac{3}{4}$ ounces in the ſpace of half an hour; at this rate, a baſket of cokes weighing 80 lb. ſaturated with water, will contain 140 ounces of water, or $8\frac{3}{4}$ lb. If the charge contains ſix baſkets, then we ſee that upwards of 50 lb. of water is introduced regularly along with the charge, furniſhing an additional quantity of oxygen equal to $42\frac{1}{2}$ lb., and of hydrogen equal to $7\frac{1}{2}$ lb.: it frequently happens that the cokes contain a larger portion of water than is here ſtated. When cokes thus ſurcharged are introduced in quantity into the blaſt-furnace, the quality of the metal is not always inſtantaneouſly changed, and frequently the colour and form of the cinder remain long without any great alteration. The contact of wetted cokes with the ore is firſt ſeen by the great diſcharge of pale-blue gas, with the whiter flame at the top of the furnace; next, the accumulating oxyde upon the ſurface of the pig when conſolidating indicates their preſence. Iron thus oxygenated frequently exhibits, while fluid, that agitation and delicate partings peculiar to carbonated metal: the remelting of this iron is never attended with advantage, and is always unprofitable to the founder.

From the properties aſſigned to pit-coal in this and in former papers, the following facts may be deduced:—That charcoal is the baſis of the manufacture of crude iron; that its proper application produces the moſt valuable qualities of pig-iron; that, by diminiſhing its relative proportion, or contaminating its quality by heterogeneous mixtures, the value and fuſibility of the metal is loſt; but that, by a proper increaſe,

creaſe, and always in proportion to this increaſe will the fuſibility and value of the iron be mended. From the whole an important leſſon may be learned of the pernicious effects of water in the furnace, and how abſolutely neceſſary it is to prepare the cokes without uſing water, either to damp the fires, as in the uſual mode, or to cool the cinders obtained from the tar kilns, to prevent their conſuming in the open air: in all this hurtful operation conſiderable quantities of water become fixed in the cokes, which require a very great degree of heat to expel.

The preparation of iron-ſtone has already been fully attended to, and the phenomena which it exhibits under every ſtage minutely deſcribed. In conſequence of various experiments we are authoriſed to draw the following concluſions: That when pure calcareous iron-ſtone is uſed, it admits of having the local quantity of cokes diminiſhed; that argillaceous requires a larger portion than the calcareous genus; and that ſiliceous iron-ſtone requires a greater proportion of fuel than any variety of the former genera. We have alſo ſeen that fuſibility, either connected with ſtrength or otherwiſe, is derived from the mixture of the ores; and that exceſſive brittleneſs, intimately connected with infuſibility, is alſo derived from the ſame ſource. From a review of theſe facts, we are forcibly impreſſed with the importance of combining the prepared iron-ſtones with proportions of fuel ſuited to their various natures, in order to produce all the varieties of crude iron with the greateſt poſſible œconomy. Contemplating farther the ſame ſubject, it is eaſy to be conceived that a want of knowledge of the component parts of iron-ſtones, and the effects which individually they produce, muſt lead to great uncertainty of operation in the ſmelting proceſs, wherein the beautiful œconomy of nature, and even real property, will be often unprofitably ſacrificed to precedent.

Beſides the above cauſes of alteration, dependent upon mixtures of the earths, the exiſtence of oxygen in various quantities in the ores ought never to be overlooked in proportioning the cokes to the iron-ſtone. This powerful agent, whoſe

whoſe form and ſubſtance conſtantly eludes our viſion; whoſe exiſtence is only aſcertained by the wonderful changes produced by its various combinations with the iron; and whoſe preſence in the ſame iron-ſtone, in various quantities, may produce ſuch variety of reſult as to characteriſe the ores, as containing *good* or *bad* iron, ſurely forms the moſt intereſting mixture which ores or iron-ſtones poſſeſs. It will be a momentous epoch in the manufacture of iron when the exiſtence of ſuch a principle ſhall be fully admitted by the manufacturer, and its agency, from certain viſible effects produced, adopted to explain its accompanying phenomena. Till that period he will not perceive the utility of aſcertaining the quantity of oxygen, and deviſing œconomical methods of taking it from the ore. An attention to this powerful principle can alone root out thoſe prejudices ſo inimical to the real intereſts of the manufacturer, and which ſeem to glance at Nature, as having improvidently combined her moſt uſeful metal with mixtures which could reſiſt the ingenuity of man, or ſet his comprehenſive intellect at defiance. In the progreſs of this great inquiry, is it not poſſible that the preſent expenſive exertions may in part be ſuperſeded? Is it not poſſible, that, by laying open the ſources of information to individuals at large, a greater maſs of intellect may engage in the practice of this art? While the preſent extenſive and lofty buildings are neceſſary, the buſineſs is entirely confined in the hands of men of great capital: the extent of their manufactures require that a large tract of country be devoted to their ſupply; a natural conſequence is, that innumerable ſmall tracts of land are overlooked, or held unworthy of notice, merely becauſe they cannot, in a period neceſſary to clear a great capital and inſure a fortune, afford the neceſſary ſupply of materials. Such ſituations, according to the preſent ſtate of the iron buſineſs, muſt remain unexplored. Should, however, a deſire for truth once gain footing in the manufactories of iron, and ſhould this natural impulſe of the unprejudiced mind keep pace with other branches of intellectual information, we may not deſpair of ſeeing many imperfections removed, which were the unavoidable conſequence of the period of their creation.

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In the application of iron-ſtone in the blaſt-furnace, the following particulars ought rigorouſly to be attended to:—

1. Their mixtures, whether clay, lime, or ſilex: their relative proportions to each other, judging according to the rules formerly laid down; which of them may admit of a diminution of fuel; which of them will afford the quality of iron at the time requiſite; and which of them will be moſt likely, by a judicious arrangement, to give the greateſt produce of metal, united with value and œconomy. Iron-ſtones, united with large portions of ſilex, have already been ſtated to require a greater proportion of fuel to carbonate their metal than the other genera. When ballaſt or forge-pigs are wanted, it ſtands to reaſon that ſiliceous iron-ſtones ought to be uſed; not that they contain a greater quantity of iron, but becauſe they form a ſubſtitute for the other kinds, which may be more advantageouſly ſmelted for the production of more valuable qualities.

2. The quantity of metal which each individual iron-ſtone may contain, is another object of conſideration. Beſides the proportion of mixtures, which chiefly contribute to the fuſibility of iron-ſtones, a ſecond degree of fuſibility is dependent upon the richneſs of the ore in iron: this is ſo obvious in the uſe of the Cumberland and Lancaſhire ores, that the conſequences of their introduction will be perceived, by the change of the ſcoria and metal, in half the time that change would be effected by ordinary iron-ſtones. It has been frequently noticed, that crude iron contained pure carbon in proportion to its fuſibility; then the more fuſible, or ſupercarbonated qualities, muſt take up, comparatively, a conſiderable portion of the carbonaceous principle from the fuel. From this reſults a ſtriking conſequence, that the quantity of fuel ſhould, over and above its relation to the mixtures, bear a juſt proportion to the quantity of iron in the ſtone: for example, let the weight *per* charge of fuel at a blaſt-furnace be 400 lb., and let this be ſuppoſed ſufficiently to fuſe and carbonate the iron contained in 360 lb. of iron-ſtone; let the quantity of metal be ſuppoſed 35 *per cent.* then the produce will be 126 lb. Should a change take place, and iron-ſtone richer in iron be applied, though the ſame by weight, and ſhould this iron-

ſtone

ſtone yield of torrefied ſtone 45 *per cent.* its produce will be 162 lb. or 40 lb. more than the former. As there exiſts no greater proportion of carbon in the furnace, it is evident that the exiſting quantity, being diſtributed over nearly 1-3d of more metal, muſt therefore be in more ſparing quantity in the whole, and the value of the metal conſequently reduced.

3. The weight of oxygen contained in iron-ſtones is the next object of ſerious conſideration. I have already ſhewn, from experiment, that our iron-ſtones naturally contain from 9 to 14 *per cent.* of oxygen, which remains after torrefaction; it has alſo been ſhewn, that this quantity of hurtful mixture may eaſily be doubled by over-roaſting or under-roaſting the ſtone; and that the bad effects entailed are in the ratio of its combination with the iron. From a review of the facts adduced on this ſubject in various parts of my papers, its agency and effects will eaſily be credited by men of ſcience; its property of conſtituting the acidifying baſe of all the acids readily explains the unalienable conſequence of its preſence with acidifiable baſes. The effects are ſtill more pernicious when the oxygen is furniſhed by the decompoſition of water in raw iron-ſtone; the hydrogen in this caſe ſet free, alſo ſeizes a portion of the carbon; and theſe abſtractions, united to that produced by the native portion of oxygen in the ſtone, form an aggregate which frequently reduces the value of iron 40 *per cent.* So long as the principles of ſcience are overlooked in the manipulations of the foundry and forge, the exiſtence of ſuch agents will be treated as chimeras of the philoſopher and chemiſt, and the effects hourly produced by them induſtriouſly attributed to cauſes which in point of unity or conſiſtency will not bear the ſlighteſt touch of inveſtigation.

XI. *A new Theory, pointing out the Situation of the Magnetic Poles, and a Method of discovering the Longitude. By* P. R. NUGENT, *Esq. formerly Surveyor-General of Lands for the Island of Cape Breton. Communicated by the Author.*

THE several interesting and important voyages, undertaken by order of his present Majesty, for making discoveries highly beneficial to mankind, particularly in what relates to geography and navigation; and the great desire and intention of deriving every possible benefit from the many observations which such extended voyages were designed to offer ample room and opportunity for making, became objects of peculiar concern and consequence. In addition, therefore, to the undoubted and acknowledged abilities of the respective commanders and officers appointed to such designations, the Commissioners of the Board of Longitude thought fit to appoint, for every voyage, one or more persons on whose scientific ability, fidelity, and diligence in assisting to make the respective observations, they could also rest satisfied. These gentlemen were therefore furnished with the completest instruments which the most scrupulous care, intention, and circumspection, together with a total disregard to expence, could be found to procure. Their report and testimony is therefore conclusive proof of the perfection or imperfection of the several instruments they had with them; their report of the azimuth compass, and dipping-needle or compass, is as follows:—

Of the Azimuth Compass.

The late ingenious and accurate the Hon. Captain Phipps, afterwards Lord Mulgrave, in his account of his Voyage towards the North Pole, p. 108, remarks as follows:—"The variation of the compass, always an interesting object to navigators and philosophers, became peculiarly so in this voyage, from the near approach to the Pole: many of the theories that had been proposed on this subject were to be brought to the test of observations made in high latitudes, from which alone

alone their fallacy or utility could be diſcovered; they of courſe engaged much of my attention, and gave me the fulleſt opportunity of experiencing, with regret, the many imperfections of what is called the azimuth compaſs. This inſtrument, though ſufficiently accurate to enable us to obſerve the variation ſo as to enable us to ſteer the ſhip without any material error, with the precaution of always uſing the ſame compaſs by which it is taken, is far from being of ſuch a conſtruction as to give the variation with that degree of preciſion which ſhould attend experiments on which a theory is to be founded, or by which it is to be tried. The obſervations taken in this voyage will fully evince this by their great variations from one another in ſhort intervals of time; nor is this diſagreement of ſucceſſive obſervations peculiar to high latitudes, and to be attributed to a near approach to the Pole, as I found it take place even upon the Engliſh coaſt."

Mr. Wales, F. R. S. Maſter of the Royal Mathematical School in Chriſt's Hoſpital, Secretary to the Board of Longitude, &c. in page 49 of the Introduction to the Original Aſtronomical Obſervations made in the Courſe of a Voyage towards the South Pole, and round the World, in his Majeſty's ſhips the Reſolution and Adventure, in 1772, 1773, 1774, and 1775, by himſelf and Mr. William Bayley, now Maſter of the Royal Academy at Portſmouth, publiſhed by order of the Board of Longitude, ſays:

" I cannot paſs over this article without making a remark or two on the irregularities which we found in the obſervations made with theſe inſtruments in the Channel of England: the extremes of the obſerved variations were from $19\frac{3}{4}$ to 25; and all the way from England to the Cape of Good Hope I frequently obſerved differences nearly as great without being any way able to account for them, the difference in ſituation being by no means ſufficient. Theſe irregularities continued after leaving the Cape," *i. e.* they continued throughout the voyage. But theſe irregularities are trifling compared with thoſe

Of the Dipping Needles, or Compaſs.

Theſe inſtruments were ſo imperfect in principle, and the obſervations made with them ſo uncertain, that, excepting

the obſervations of the magnetic inclination, made at anchor or on ſhore, (and even theſe, as any one may perceive, were far from being certain,) no manner of uſeful certitude or judgment could ariſe from them. Mr. Wales, in his deſcription of the dipping needle here ſpoken of, and made by Mr. Nairne agreeable to a plan of the Rev. Mr. Mitchell, F. R. S. obſerves, in page 50 of the Introduction aforeſaid:

"The principal defects in this conſtruction are, the difficulty in placing the wires which carry the two laſt mentioned balls in the proper plane, and the total impoſſibility of knowing when they are ſo: moreover, it is very poſſible, and undoubtedly often happens, that the axis of the needle and its two poles do not lie in the ſame plane, in which caſe another difficulty will ariſe in adjuſting the needle to great exactneſs." And in page 15 of the Obſervations themſelves, Mr. Wales remarks as follows:

"The dipping needle which we took aſhore at this place (the Cape of Good Hope) was ſo much out of balance, and ſo difficult to get in again, that notwithſtanding we both of us (Mr. Wales and Mr. Bayley) ſpent all the leiſure time we had from other obſervations, we did not get it perfectly adjuſted before we went away, and of courſe were not able to get any obſervations of this kind at this time."

Mr. Bayley, in page 217 of the Original Obſervations made in the courſe of a voyage to the Northern Pacific Ocean, for the diſcovery of a north-eaſt or north-weſt paſſage, in his Majeſty's ſhips the Reſolution and Diſcovery, in the years 1776, 1777, 1778, 1779, and 1780, after, as appears, much precaution on obſerving with, and changing the poles of the dipping needle, &c. ſays: On the 28th July is mentioned an accident happening to the dipping needle: "the poles were not then changed, as the obſervations afterwards appeared regular; neither were the poles changed on receiving it on board, as it was ſaid to be well balanced: but the not doing it at either of theſe times was a great overſight, as we do not know the error of the dips with the marked end north, and dipping in any obſervations before to-day, and therefore cannot correct them according to Mr. Cavendiſh's directions. If we ſuppoſe the ſame error from the firſt as on to-day, (the mean

mean dip with the marked end north, exceeding the mean dip with the marked end south by 8° 8′), then are the mean dips from the beginning to be diminished by $\frac{488}{2}$ or 244 × cosine of the dips. If, again, we suppose no error on receiving it, nor from the above accident, the mean dips to this day will be lessened by $\frac{244}{2}$ or 122 × cosine of the dip."

What error or difference arose between the extreme of the observed dips with the marked end north, and those with the marked end south, are no where set down; they must, however, undoubtedly have been greater, and in all likelihood much greater than 8° 8′, seeing the mean result of the whole, small and great, gave 8° 8′. The observations made by Lord Mulgrave are of the like nature and uncertainty.

The whole of these instruments were therefore totally inadequate to the correct and useful purposes of navigation, or indeed to any correctly useful purpose whatever: they were however, as before observed, the very best that could be obtained; and in the making of which no expence whatever on the part of the Board of Longitude, nor pains on the part of those who made them, were wanting to render them as perfect as possible, and such as should answer the purposes for which they were designed; nor do I entertain any doubt they were as much so as the nature of their construction could possibly admit of.

My pursuits in life having been considerably connected with objects of this kind, my own experience, corroborated by the above and like testimonies, together with the continual necessity, which arose from time to time, of increasing the magnetic orbit so as to correspond to the increase of the variation (on the supposition that the true dip and variation might be accounted for and predicted,) by means of moveable magnetic poles *, led me into endeavours to investigate

* William Whiston, M. A. in the 8th page of an historical preface to a pamphlet of his, entitled, "The Longitude and Latitude found by the Inclinatory or Dipping Needle," gives us the brief history of all theories then published, in these words, speaking of the magnetic poles and theories:

"However, this notion in general of moveable magnetic poles has always

tigate ſome theory more conformable to the laws of Nature*, and which would reconcile the continual diſagreement between men of ſcience on this ſubject: and this deſire alſo of neceſſity led me into endeavours to form more perfect nautical inſtruments, for without more accurate obſervations no eſſential utility whatever could ariſe to navigation from any kind of theory however perfect. To detail the train of reaſon-

ways ſeemed ſo probable, or rather neceſſary, ever ſince the variation has been itſelf found to vary, and this after a certain regular manner; alſo, that Mr. Phillips before Mr. Bond aſſerted the ſame, and ſtated the revolution to be in 370 years; and after all, our learned Dr. Halley, who has far outdone every body upon this ſubject, has determined it to be ſo, only he has thought himſelf obliged to add the hypotheſis of two other fixed poles; and from the joint effects of all four poles, and from thoſe only, has he been able to bring this variation of the variation to ſome kind of ſyſtem agreeable to the obſervations. He has alſo been obliged to lengthen the period of the moveable poles' revolution; and as Mr. Bond had enlarged Mr. Phillips's number from 370 years to 600, ſo has Dr Halley enlarged the ſame farther, from 600 to 700 years;" and Mr. Whiſton, in page 58 of the pamphlet itſelf, ſtates the revolution of Dr. Halley's inner nucleus, or north magnetic pole, to be in 1920 years, which, he obſerves, is much ſlower than that of Mr. Phillips, Mr. Bond, or Dr. Halley.

Since Whiſton's time, thoſe who attempted the magnetic theories are: the celebrated Euler, who, as all others have hitherto done, ſuppoſes that the magnetic poles move; Mr. Lorimer, Mr. Churchman, Mr. Walker, &c.; but of all theſe, Mr. Churchman is the only perſon that determines their revolution, which he ſtates to be, the northernmoſt in 1096 years, and the ſouthernmoſt in 2289 years.

I ſhall forward, for a future number of the Philoſophical Magazine, a more full account of the theories that have hitherto prevailed, in which I ſhall enter more fully, than my preſent limits will allow, into the truth of my new theory, and of the method by which I have been enabled to prove the fallacy of the former. In the mean time any perſon, even by roughly peruſing the obſervation of latitudes, longitudes, and dips, made on the north-weſt coaſt of America, and comparing them with that at London, and alſo with thoſe made off the eaſt coaſt of South America and in the Chineſe ſeas, will ſoon be enabled to aſcertain, ſufficiently near for the purpoſe of remaining ſatisfied, that the ſituation of the magnetic poles is as deſcribed: my determinations are, however, drawn from a multitude of calculations, and the reſult compared with all the magnetic dips and variations (taken in all the different places on ſhore) I could procure.

* I account for the change of the magnetic variation by *original magnetic meridians* and a *magnetic annulus*. N.

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ing and consequent investigation I went through, would be unnecessary here. The result, which I have long since explained to many individuals, I now, in as brief a manner as the subject will admit of, lay before the public, not doubting that an object, which actually tends to the enlargement and perfection of science, and, in my opinion, to the benefit of navigation, will find favourable reception, investigation, and experiment.

Method of discovering the Longitude, the Magnetic Inclination and Latitude of the Ship being given:

PROBLEM the First, being the THEORY.

To determine the Latitude and Longitude of the Magnetic Poles, and, in consequence, for the purpose of discovering the Longitude, to ascertain a First Meridian.

Let N (Plate VIII.) represent the north pole of the earth, M the magnetic north pole, S Smeerenberg harbour in Spitsbergen, and L London: also, let arches of great circles be drawn from London to the north magnetic pole, to the north pole of the earth, and to Smeerenberg, and also from Smeerenberg to the north magnetic pole. There will thus become formed three spherical triangles, which are resolved as follows:—

1st, In the triangle NLS are given the sides NL and NS, being the co-latitudes of London and Smeerenberg, and the included angle LNS being their difference of longitude, to determine the angle of position NLS and the third side LS.

2d, In the triangle LSM are given the three sides LS, (just found) LM and SM (LM and SM being the magnetic polar distances corresponding to the correct dips at London and Smeerenberg), to determine the angle MLS, from which subtracting the angle NLS lately found, there remains the angle of position NLM.

Lastly, In the triangle MLN are now given the sides LM and LN, and the included angle MLN, to find the angle LNM, the longitude of the north magnetic pole (and the meridian of the earth passing through the magnetic poles and

and poles of the earth,) from the meridian of London, and the ſide M N the diſtance of the magnetic pole from the pole of the earth.

The place of the magnetic poles being thus obtained, the queſtion next divides itſelf into two points of view, namely, Whether the magnetic poles or points thus determined are ſtationary or otherwiſe? the ſolution of which depends on obſervation and philoſophic inference. From obſervation it is remarked, in page 121 of the late Lord Mulgrave's Account of his Voyage towards the North Pole, that—

"There is no reaſon at preſent to ſuppoſe that the dip is liable to any variation in the ſame place at different periods of time. It having been obſerved at London by Norman, who firſt diſcovered it in 1592, to be 71°.50′, and by Mr. Nairne, in 1772, about 72°: the difference between theſe obſervations, taken at ſuch diſtant periods of time, is ſmaller than that found between ſeveral of Mr. Nairne's obſervations compared with each other; and therefore we have no reaſon to conclude that the dip has altered ſince Norman's time. The care with which his inſtrument was conſtructed, and his obſervations made, leave us no room to doubt of their accuracy."

Again, Mr. Cavallo, in his Treatiſe on Magnetiſm, p. 65, remarks, (ſpeaking of the dip,) "Its alteration in the ſame place at different times is very ſmall; thus in London, about the year 1576, the north pole of the dipping needle ſtood 71.50 below the horizon, and in the year 1775 it ſtood at 72.03: the alteration of the inclination in ſo many years amounting to leſs than a quarter of a degree, which may be attributed to the error of the inſtruments, ſince, as will appear in the ſequel, thoſe inſtruments are far from having attained to any degree of perfection even in the preſent age."

Now, from philoſophic inference it will naturally follow, that if the inclination of the magnetic needle at different periods of time at any one place remain conſtant, the magnetic poles are ſtationary; and if the magnetic poles are ſtationary, (as appears clearly evident from above,) the meridian thus diſcovered is the *Firſt* (or only) *Meridian* from which the longitude

gitude ought to be counted; I have always used it as such, and obtained the most satisfactory results.

PROBLEM the Second, being the PRACTICE.

To determine the Longitude universally.

Here are always given the ship's polar distance, the magnetic polar distance, and the magnetic co-latitude, being three sides of a spherical triangle, to determine the angle at the pole of the earth opposite the magnetic co-latitude, being at all times and places the longitude of the ship east or west (as the case may be) from the meridian aforesaid.

N. B. The magnetic co-latitude is the distance of the ship from the nearest magnetic pole; the ship's polar distance is the distance of the ship from the pole of the earth next adjoining the said magnetic pole; the polar distance is the distance between either magnetic pole and the pole of the earth nearest thereto: moreover, the longitude thus obtained can never exceed 180. It may, however, be easily reduced as to that estimated from any assumed first meridian. Likewise the point M, thus assumed as the magnetic pole, may, to prevent perplexity, be better understood as being that point on the surface of the globe at which the dipping needle stands at right angles to the plane of the horizon, without regarding whether the magnetic poles themselves are under, at, or above the surface of the earth.

The method of obtaining and establishing (from observation alone) certain data for investigating or corroborating perfect tables of magnetic latitude and co-latitude, appears (from the preceding theory) obvious, being, by carefully making different dips of the magnetic needle along the meridian aforesaid, and carefully observing the respective latitudes at which those dips are taken, and for greater certainty (if thought needful) continuing them, as far as possible, into both hemispheres; though it will also be perceived they may be investigated by observing different dips, along any other great circle, passing through the magnetic poles, or having determined the precise latitude and longitude of the magnetic poles. Correct tables may also be deduced from correct dips at various places; however, the

firſt method is not only the moſt eaſy, but on many accounts the moſt preferable; and for this purpoſe the correct dips of the magnetic needle, taken at the following places, (regard alſo being had to determine their correct latitudes and longitudes, particularly the former, except near the magnetic poles, where it is neceſſary to correctly determine both,) will be found ſufficient, namely, at the Falkland Iſlands, Buenos Ayres, Oroonoque, Trinidad, Barbadoes, Saint Luke, Martinique, Guadaloupe, Antigua, Saint Martin's, Bermudas, the Atlantic Ocean, in or near the ſame longitude up to and near the Iſle of Sables, at the Iſland of Cape Breton, the ſouth ſide of the river Saint Lawrence in the longitude aforeſaid; then going round to the north-eaſt ſhore of the Labrador Coaſt, and obſerving thereon at Davis's Inlet, and ſo on up to the northward under the meridian aforeſaid, up to and into Baffin's Bay, and there carefully determining the latitude and longitude of the north magnetic pole; or firſt going into Baffin's Bay, and afterwards proceeding to the ſouthward.

Next, by carefully obſerving the magnetic inclination along the oppoſite meridian in Ruſſia, China, the Chineſe Seas, Indian Ocean, &c.; as at the mouth of the Leno, at Olikſminſkoi, at Nutſchink or Albazin, at Pekin, Nankin, Nimpo, Formoſa, the Philippine Iſles, Gindano, Celebes, Flores, the north and ſouth ſide of New Holland, and ſo continuing to obſerve along the Indian Ocean, in or near the meridian aforeſaid, up towards the ſouth pole of the earth, until the place of the ſouth magnetic pole be carefully determined, *i. e.* whatever pole may be firſt convenient to determine. The number of places herein ſet forth are mentioned in order that navigators and others, as opportunity may offer, may know where and how to make the advantageous obſervations herein alluded to.

It may alſo be proper to add, that hitherto the places of the magnetic poles have always been attempted to be found by means of the magnetic variation; a method whereby the moſt able mathematicians have been continually led aſtray, for, as the magnetic variation is continually changing, their concluſions

conclusions were rendered as uncertain as the variation itself, of which (it had so happened from the method they pursued) they knew little more than that it did change. Had the present theory occurred to them, it would have received chearful and favourable countenance and encouragement, and it and its utility been soon and effectually established. Had the indefatigable and famous Dr. Halley in particular known thereof when he undertook two voyages (long after the inclination of the magnetic needle was discovered, namely, in 1698 and 1699) for the express purpose of endeavouring to form a magnetic theory by means of a multitude of observations of the magnetic variation, as a preparatory step, and in order to determine the longitude thereby, or, as his instructions run, "to seek, by observation, the rule of the variation of the compass;" the observations of magnetic dip here recommended, and which he would have made at least along the western line before pointed out, together with the observations of the variation of the compass, which he did observe, in all likelihood, if made with instruments capable of affording the requisite accuracy, would have soon suggested, to his penetrating and fruitful genius, the correct object of his research.

It may be farther proper to remark, that the observations of the magnetic inclination herein stated, and made at London, do of themselves alone establish another important conclusion, likewise quite different from the general opinion of the Learned, which supposes that the magnetic variation affects (*i. e.* increases or diminishes) the magnetic inclination; for, from the time in which the magnetic inclination was first observed at London to the present time, the variation has shifted upwards of 36° without affecting the inclination; and therefore, after knowing the result of so very great a change in the magnetic variation without having affected the inclination, there can be no reason to conclude that it can in any respect be affected thereby: a circumstance that renders the theory here pointed out still more simple and beautiful.

Thus, by new and interesting application and testimony of unerring science, practical observation, and fair conclusion,

I have, I flatter myſelf, cleared the magnetic theory of the abſtruſe and embarraſſing conſiderations which have hitherto bewildered it, and thereby laid the foundation of a moſt ſimple, general, and uſeful method of determining a ſhip's correct place at ſea, without any regard to the ſun, moon, or ſtars, to good or bad weather, to the time of the day, the day itſelf, months, years, ſeaſons, or centuries, except ſo far as may be ſaid to regard the magnetic inclination, which muſt always be known from obſervation; as alſo the latitude of the ſhip, which muſt likewiſe be known, either from obſervation, or dead reckoning.

I would not have it underſtood, however, that I mean to decry the uſe of lunar obſervations or chronometers. Far otherwiſe: I have devoted great application to the ſtudy of the lunar (and other) aſtronomy; and herein I am happy to add, I have ſucceeded to an accuracy and conciſeneſs of equations and calculations far ſurpaſſing Mr. Mayer's, or any lunar tables yet offered to the public. But herein, notwithſtanding the high eſteem and veneration in which the great genius and labours of Euler, and the merits of Mr. Mayer muſt ever be held by men of ſcience, I found it neceſſary to reject, for reaſons which I ſhall hereafter tranſmit for inſertion in the Philoſophical Magazine, the ſuppoſition that the motion of the ſun or earth, according to Mr. Euler, and of the moon according to Mr. Mayer, are different now to what they formerly were. I am alſo the original inventor, and for which I have, for upwards of ſeven years, had a patent, of a double ſextant for making lunar obſervations to the right and left. I have alſo invented a metal quadrant, by which, with fore-adjuſtments and obſervation, the diſtance of the moon can be meaſured from the ſun or other object, from one limit or extreme point of the horizon to the other.

From the multitude of lunar places which I have calculated from my tables, I am perſuaded they are ſuch as ſhall not, even at the equator, produce a mean error of five, nor an extreme error of fifteen miles in longitude. The firſt of the inſtruments here ſpoken of (a double ſextant) meaſures with convenience, without inverting the inſtrument, the ſimple limits of a ſextant on either ſide the ſun or a fixed ſtar:

to the other there can be no limits, for it meaſures 180 degrees on either ſide. Not ſatisfied with doing thus much, in order to avoid the inconvenience, trouble, and inaccuracy ariſing from the uſe of large volumes of charts of ſpherical triangles, and of tables for reducing obſerved lunar diſtances to true, I contrived an inſtrument, for which, as well as ſeveral ſuperior ſurveying and other inſtruments, I have alſo got a patent. This inſtrument, which I have denominated a ſuit of circles of calculation, performs this operation with eaſe, accuracy, and expedition. It alſo determines the true latitude, without knowing the latitude by account, either by double altitudes and the time between, by double altitudes and the difference of magnetic azimuths, (which my inſtruments will correctly give,) or by ſimply having the altitude of two known fixed ſtars. It determines the time of the day, &c. &c. In a word, it reſolves all manner of ſpherical triangles or ſpherical trapezia, &c. The reſolution of the few of theſe that conſtitute the eſſentials of nautical aſtronomy and geography, becomes by this means obvious, eaſy, and pleaſant; whereas the underſtanding, or even the appearance of the previous burthen of requiſites, creates, as is well known, a great if not inſuperable obſtacle to the generality of otherwiſe well-informed ſeamen.

Having thus, I truſt, explained my regard and eſteem for the lunar obſervations and chronometers, all I would have underſtood of the method of diſcovering the longitude by the dip is, that it is eaſily underſtood by the moſt ordinary capacity; that to others, in every inſtance, it becomes an auxiliary or corroborating proof, and, in the time of greateſt neceſſity and peril, a moſt eſtimable ſubſtitute; not requiring, in this caſe, any calculation whatever. Thus, from the preceding theory, the dip in all places remaining the ſame, the navigator, being perhaps ſeveral days without ſeeing ſun, moon, or ſtars, ſo as to make any uſe of either towards determining his longitude, has this benefit left him, that he can, at any moment of the day, in which the extremity of the horizon can be ſeen, determine his correct dip, and therefore his vicinity towards the land he wiſhes to make or avoid; where otherwiſe he might either be caſt away, or, under the

the ſuppoſition that he was near thereto, keep either lying-to, or ſtraining his ſhip in endeavouring to get no farther to leeward, when at the ſame time he might be an hundred miles or more from ſhore, and ſo loſe the very wind he wanted; after which, ſhould contrary winds aſſail him, the injury alſo becomes obvious.

To clear apparent or obſerved Lunar Diſtances from the Effects of Refraction and Parallax by the Suit of the Circles.

Set the vernier on the firſt meridian, carrying the circle or ſemicircle of poſition to the apparent altitude of the ſun's centre, or that of the ſtar; the vernier on the moveable meridian to the apparent altitude of the moon's centre; and the vernier on the circle or ſemicircle of poſition to the apparent diſtance of the ſun and moon's centres, or of the centre of the moon and fixed ſtar. Let theſe form a ſpherical triangle; ſo ſhall the number of degrees, minutes, and ſeconds on the equator, comprehended between the firſt and moveable meridian, ſhew the angle (or difference of azimuths) at the vertex, which angle is common to the apparent and true triangle. Keeping faſt, therefore, the ſaid meridians by means of the equator, ſet the vernier on the firſt meridian to the true altitude of the ſun or ſtar's centre, and the vernier on the moveable meridian to the true altitude of the moon's centre; then ſee what number of degrees, minutes, and ſeconds of the circle or ſemicircle of poſition or diſtance are contained between them, for that is the true diſtance.

Portſmouth, Dec. 13, 1795.

We certify, that, in our opinion, a compaſs and ſextant invented by Mr. Nugent merit an expeditious and accurate inveſtigation and trial.

(Signed) T. Packenham, *Le Juſte.*
J. Craunston, *Bellerophon.*
E. Gower, *Triumph.*
W. Domett, *Royal George.*
R. Bowen, *Terpſichore.*

The foregoing certificate was given to me, unſolicited, for the purpoſe of being tranſmitted to the Lords Commiſſioners of

of the Admiralty, in order to procure their Lordſhips order to have the merits and efficacy of my inventions and inſtruments officially examined into and reported upon. As to the inſtruments, they were on the ſame day applied for, through Commiſſioner Marſh, to the Navy Office, by the above gentlemen.

The moſt ſpeedy and leaſt expenſive method of aſcertaining the preciſe ſituation of the north magnetic pole, the ſouthern being oppoſite thereto, appears to be, by engaging one of the Davis's Strait whalers to proceed into Baffin's Bay, with a proper perſon duly qualified, to make the neceſſary aſtronomical and magnetic obſervations; or rather, by diſpatching for this purpoſe a frigate or other veſſel in the king's ſervice employed on the Newfoundland ſtation. Pity it was, indeed, that the Board of Longitude had not known of this method, and of the reaſons that render all navigation into the South Sea, by any paſſage to the eaſtward of Greenland, totally impracticable. The Hon. Captain Phipps's deſtination would in that caſe have been to proceed into Baffin's Bay, where, had he alſo poſſeſſed the angular and magnetic inſtruments herein alluded to, he would have aſſuredly aſcertained one great object of his voyage, namely, the determining the fallacy or utility of all previous theories, and bringing them to the teſt of experiment; a teſt which would have cauſed the whole to have vaniſhed, and none other would have remained but that I have already pointed out. Nor do I entertain much doubt that, by attending to circumſtances, which might eaſily be pointed out, Captain Phipps would have alſo accompliſhed a paſſage into the South Sea.

The principles of the whole of my nautical magnetic inſtruments (except a variation frame for converting all ſteering into variation compaſſes) is the application of a quadrant, ſextant, or double ſextant, (fitted for this purpoſe,) to the vertical magnetic axis of the horizontal needle for determining the variation, and to the lateral magnetic inclinatory axis of the dipping needle for determining the dip. Theſe needles are alſo differently ſuſpended from others: the dipping needle, for example, is always at liberty to reſort to, and remain

at

at reſt in, the common interſection of the lines of the magnetic meridian or variation and dip; nor can any alteration of direction or motion of the ſhip affect a dipping needle thus ſuſpended.

In conſequence of this ſimple contrivance, the motion or gyration of the graduated circles for either dip or variation on account of the motion of the ſhip, becomes totally avoided; for, on or in theſe magnetic inſtruments there are no diviſions whatever: the inſtruments themſelves can be afforded for half the price of others; the navigator makes his obſervations like a man of ſcience; and finally, he determines with expedition, eaſe, and certainty, the magnetic dip and variation, to degrees, minutes, and ſeconds, *i. e.* to the like accuracy that he can diſtinguiſh or read the ſubdiviſions ſhewn by the vernier on the arch of his ſextant: a circumſtance utterly impoſſible to be obtained even to whole degrees, it might be ſaid to half a dozen degrees, by any other conſtructed inſtruments, as has been herein before ſufficiently verified.

XII. *Deſcription of a Portable Machine for loading and unloading Goods. By Mr.* GEORGE DAVIS, *of Windſor, Berks* *.

THE ingenious contriver of this machine is certain, that, when made of its intended ſize, it will be capable of loading a ton weight by one man only, and will be ſo portable as not to exceed one hundred and twelve pounds in weight.

Reference to the Figure of the Machine. (See Plate IX.)

A, the winch, which turns the bar B. This bar has on it two endleſs ſcrews or worms C, C, which work in the toothed wheels D, D. Theſe wheels are fixed to the barrels E, E, round which the ropes F, F, coil, wind up, or let out the ſame occaſionally; which ropes, paſſing over the two

* From the *Tranſactions of the Society for the Encouragement of Arts, Manufactures, and Commerce.*

A premium of forty guineas was given to Mr. Davis for this invention.

pulleys

pulleys G, G, are brought round, and their ends, having hooks for that purpoſe, are hitched into ſtaples fixed to the front of the cart or other carriage: within theſe ropes the load H is placed on a common pulley I, which forms an inclined plane, up which, by the turning of the winch, the ropes are wound upon the barrels, and the load raiſed into the carriage.

K K, the frame, intended to repreſent the part of the cart, or other carriage, on which the machine is occaſionally to be placed.

The whole of the barrels and cogged wheels are contained in an iron box L, the ſides of which are repreſented in the figure as taken off, that the conſtruction of the ſeveral parts may be ſhewn.

XIII. *Deſcription of a cheap and efficacious Ventilator for preſerving Corn on Ship-board. By* THOMAS SOUTH, *Eſq.* *.

THE importation of grain is a precarious traffic. The produce of diſtant countries, or even of thoſe near home, when long in collecting, or long detained on ſhip-board, is ſubject to heat, ſoon becomes fetid, and is often ſo far ſpoiled and depreciated in its value as to ſell for leſs than the original coſt. Hence the merchant, overwhelmed with loſſes, regrets his patriotiſm, grows ſhy of importation, and, unleſs invited by a certainty of gain, drops the trade, even whilſt the nation ſtands in need of ſupplies.

The remedy here propoſed is a ſimple, cheap, and, I truſt, efficacious method of ventilating grain whilſt confined on ſhip-board; ſufficient, I preſume, to keep it ſweet and marketable after ſuſtaining a tedious voyage.

Deſcription of the Ventilator, with References to the Figures thereof. (See Plate VII.)

Fig. 1. is a cylindrical air-veſſel or forcing-pump, of lead,

* From the *Letters and Papers of the Bath and Weſt-of-England Society for the Encouragement of Agriculture, &c.*

tin, or other cheap metal; its internal diameter being ten inches, and its length three feet; having a crutch-handled piſton to work with, and an iron noſle, *viz.* a hollow inverted cone, two feet long, to condenſe the air, and increaſe its power in its paſſage downwards. This cylinder ſhould be riveted or ſcrewed, by means of an iron collar or ſtraps, to the deck it paſſes through, both above and below, as at *a a*; and ſhould be farther ſecured by ſome hold-faſt near *b*, to keep it ſteady in working.

Fig. 2. is a bottom of wood, four inches and a half thick, with a projecting rim at its baſe, for the metal cylinder to reſt on, when cemented and ſcrewed to the wood. The centre of this bottom is excavated, for the reception of the crown of the noſle. In the ſame figure the noſle is repreſented with its crown like a bowl-diſh, to condenſe the air gradually, without reſiſtance, in its advance to the more contracted baſe of the inverted cone, *i. e.* the top or entrance of the noſle. About two-thirds down this noſle may be fixed a male ſcrew, as *c c*, for the purpoſe hereafter mentioned.

N. B. The forcing-pump ſhould be caſed in wood, to protect it from outward bruiſes, which would prevent the working of the piſton, and ruin its effects. The leather round the embolus ſhould be greaſed when uſed.

Fig. 3. is a crutch-handle, faſtened to the embolus A by its iron legs B, B. A is a cylinder of wood, caſed with leather, ſo as to fit well, but glide ſmoothly in the metal cylinder; having an opening as large as its ſtrength will permit, for the free acceſs of atmoſpheric air. C is a valve, well leathered on its top, and yielding downwards to the preſſure of the air when the piſton is raiſed up. D is a croſs bar of iron, to confine the valve, ſo that it may cloſe inſtantly on the return of the piſton downwards.

Fig. 4. is a tin pipe or tube, of leſs than four inches diameter, and of ſuch length as, when fixed to the baſe of the cylinder, Fig. 1, ſhall admit the noſle *d*, Fig. 2, to within half an inch of the valve E, at the bottom of the wooden cylinder F, in Fig. 4; which valve E will then yield to the preſſure of air condenſed in its paſſage through the noſle, and deliver it into the pipes below. This valve muſt be well leathered

leathered on its upper ſurface, and faſtened with an hinge of leather to the cylinder it is meant to cloſe: affixed to its bottom is the ſpindle G, paſſing through a ſpiral ſpring H, which, being compreſſed on the deſcent of the valve, will, by its elaſticity, cauſe it to riſe again, cloſe the aperture above, and retain the air delivered beneath it. On connecting this cylinder with the upper end of the noſle, at *e e*, Fig. 2, we muſt carefully prevent any lapſe of air that way, by a bandage of oakum ſmeared with wax, on which to ſcrew the cylinder, like the joints of a flute, air-tight. I is a bar of iron, having a riſing in its centre, wide enough for the ſpindle to play through, but at the ſame time ſufficiently contracted to prevent the paſſage of the ſpiral ſpring.

Fig. 5. is an aſſemblage of tin pipes, of any lengths, ſhaped ſuitably and conveniently to their ſituation in the ſhip, to the form of which, when ſhut into one another, they muſt be adapted; obſerving only, that the neck be ſtraight for a length ſufficient to admit the lower end of the cylinder, Fig. 4. as high as the letter F, or higher.

Fig. 6. To the middle pipe, which runs along the bottom, ſhould be fixed a perpendicular one, fully perforated, to convey the air more readily into the centre of the heap; and this may have a conical top, as repreſented in the plate, perforated with a ſmaller punch to prevent the air from eſcaping too haſtily. In large cargoes, two or three of theſe perpendiculars may be neceſſary; and each ſhould be well ſecured by an iron bar *g*, ſcrewed down, to prevent their being injured by the ſhifting of the cargo in ſtormy weather or a rolling ſea. The top of the conical cap of theſe pipes may reach two-thirds up the cargo.

Fig. 7. is a valve of the ſame conſtruction as that repreſented in Fig. 4, but incloſed in a tube of braſs, having a female ſcrew at *f f*, adapted to the male ſcrew *c c*, on the noſle Fig. 2, and may then be inſerted into the head of the pipe Fig. 5. This will add to the expence; but, in a large apparatus, is to be preferred, as a more certain ſecurity from lapſe of air, than the junction of the tube Fig. 4, to the neck *e e* in Fig. 2.

N. B. *e e* is a neck of wood, making a part of the bottom

Fig. 2, whereon to ſecure the tube Fig. 4, when applied to the noſle. The joints of the pipes, when put together for uſe, ſhould be made air-tight, by means of bees-wax or ſome ſtronger cement, till they reach the bottom of the veſſel, when there is no farther need of this precaution. The horizontal pipes ſhould run by the ſide of the kelſon the whole length of the hold. The tin plates of which K is made, ſhould be punched in holes, like the roſe of a watering-pot, in two or three lines only at moſt, and then formed into a tube, with the rough ſide outwards. L may have four or five lines of the like perforations. M, and the reſt, ſhould gradually increaſe in their number as they advance towards the middle of the hold, and continue fully perforated to the laſt pipe, which ſhould be cloſed at its end to prevent the ingreſs of the corn. It is the centre of the cargo which moſt requires ventilating, yet air ſhould pervade the whole. Like the trade-winds, it will direct its courſe to the part moſt heated, and, having effected its ſalutary purpoſe there, will diſperſe itſelf to refreſh the maſs.

Where the hatches are cloſe-caulked, to prevent the influx of water, vent-holes may be bored in convenient parts of the deck, to be bunged up, and opened occaſionally, from whence the ſtate of the corn may be known by the effluvia which aſcend when the ventilator is working.

The power of the ventilator is determined by the ſquare of its diameter multiplied into the length of the ſtroke, and that again by the number of ſtrokes in any given time.

To find the area of a circle, and the ſolidity of a cylinder raiſed on that circle, Archimedes gives the following proportion:—

As 1 is to .785398 decimal parts, ſo is the ſquare of the diameter to the area of the circle.

And, as 1 is to .785398, ſo is the ſquare of the diameter, multiplied by the height, to the ſolidity of the cylinder.

The cubical contents, both of cylinders and tubes, are found in the ſame manner: their difference conſiſting not in ſhape, but ſolidity, the latter being hollow.

Then, to find the contents of a cylindrical veſſel whoſe internal diameter is ten inches, multiply that into itſelf, and the

the ſquare thus obtained, multiplied by .7854, will give the contents of the circle in cubic inches; which, multiplied again by twenty-four inches, or lengths of the ſtroke, being the proportion of the barrel filled with air, gives in cubic inches the amount of each diſcharge on the deſcent of the piſton. As thus:

Inches.

Internal diameter of the pump or tube 10

× 10

= 100, or ſquare of the diameter;

which, multiplied by .7854, to bring the contents of the ſquare to the contents of the circle.

Which, multiplied by the length of the ſtroke, } 78,5400 Contents of the area of the circle.
24 inches, produces 1884 cubic inches.

3141600
1570800

18849600

which, divided by 231) 1884.9600 (8.1600 gallons, which is $\frac{1}{100}$ths more than 8 gallons at a ſtroke; allow theſe decimals for waſte of air in each ſtroke; and 60 ſtrokes to be made in a minute.

viz. the number of cubic inches in a wine gallon, quotes 8 galls. 369

1386

Then - - - - - - 8 gallons diſcharged at a ſtroke,
multiplied by 60 the number of ſtrokes

amounts to 480 gallons *per* minute;
which multiplied by - - 60, the minutes in an hour, produces 28800 gallons in that time;
and that, divided by 252) 28800 (114.3 tons.

(the number of gallons in a ton, both wine and ſhip meaſure) quotes 114 tons in an hour.

.360

1080

.720

Then, ſuppoſe the area of the hold of a ſhip to be = 120 tons, and, when freighted, the interſtices between the grains, together with the area between the ſurface of the corn and the

the underſide of the deck = 5 tons = to the quantity of mephitic air confined; ſuch being the lighteſt fluid, the major part of it would, ſoon after the commencement of the operation, be forced, by the atmoſpheric air, to vent itſelf at the holes provided for that purpoſe; and the remainder of the hour being employed in the like ventilation, five tons of freſh air would paſs above twenty times repeatedly amidſt the grains, to cool, refreſh, and ſweeten the cargo. A purification thus adminiſtered once in eight-and-forty hours, would, I conceive, be amply ſufficient to preſerve the corn from taint or injury, be the voyage ever ſo tedious; and unleſs it ſhould by neglect have overheated and grown together, or ſettled too cloſe, the labour would be that of a boy only; for the dairy-girl at her churn works harder than he otherwiſe need to do at this.

My air-veſſel is, for the ſake of cheapneſs, confined to the narrow diameter of ten inches; but, as the contents of circles are proportionate to the ſquare of their diameters, by enlarging that, you increaſe their power accordingly; wherefore, by extending the diameter to fourteen inches, the contents will be nearly doubled; and, by adding ten inches more to the length of the ſtroke, you almoſt treble the diſcharge of No. 1, and obtain a power capable of ventilating a cargo of 400 tons within the hour. But the air-veſſel muſt be lengthened; the pipes at the ſame time enlarged; the metal of which the whole is conſtructed be in ſubſtance proportionable; and the labour be that of a man, or perhaps two upon occaſion.

A ventilator, on the plan and dimenſions here propoſed, would come within the compaſs, I ſhould think, of five or ſix guineas. One on the larger ſcale, cauſed by the increaſed ſubſtance of the metal, and the extra ſize and length of the pipes, might amount to twenty; which, in either, is under fourpence *per* quarter on the firſt cargo; and as they will laſt many years if well painted, and, when not in uſe, taken to pieces and put carefully by, I flatter myſelf it is an experiment well worth trial; particularly if a premium be offered to the ſhip-owner, who, by means of ſuch machine, imports his corn pure and untainted from a diſtant land.

Objections

Objections made to the ſuppoſed Effect of the Ventilator, overruled, it is hoped, by the Conſiderations which follow them.

Firſt, The holes pierced in the tin tubes which are to lie under the corn, ſeem capable of iſſuing (eſpecially if an effort be made upon them) a much larger quantity of air than the forcing-pump will ſupply in a given time. Conſequently, a given quantity of theſe holes, under a given preſſure, will be capable of iſſuing the whole ſupply of air, without any aſſiſtance from the remainder.

Secondly, If theſe poſitions are juſt, it muſt happen, that if a cargo of corn be unequally circumſtanced in relation to its permeability, the whole of the air diſcharged by the pump will iſſue through the *more* permeable parts of it, without affecting, in any degree, the *leſs* permeable ones.

Thirdly, In cargoes heated in any degree, and in thoſe infected by that worm which faſtens grains together by a web, the parts moſt affected become much more cloſe and denſely packed together than the reſt, either by the ſwelling of the heated grains, or by the web and dung of the worms which occupy the intervals between the grains.

If ſo, the parts of a cargo which require the moſt ventilation will receive the leaſt; but, in all caſes, it ſeems likely that the air diſcharged will not regularly permeate the whole of the cargo, but will paſs through the parts where the grain lies lighteſt, and leave untouched thoſe parts where it is moſt cloſely packed together.

Anſwer to the preceding Objections.

Though the holes appear numerous, they muſt be ſmall, leſt the corn gain admiſſion; and many (eſpecially of the uppermoſt) will be nearly, if not totally, ſtopped by the preſſure of the grains upon them. Beſides, the pipes which convey the air towards the centre are not meant to be ſo fully perforated as thoſe at and beyond it; and all may be ſtill leſs ſo, if in practice found neceſſary. But as the quantity of air delivered by the forcing-pump within five ſeconds

of time is equal to the contents of sixty * feet of four-inch pipe within the first minute, the air (notwithstanding the manifold perforations, obstructed as it is in meandering through a mass so nearly compacted as the bottom of the cargo must necessarily be by the pressure of the heap above) will undoubtedly reach to the end of the pipes, and consequently affect the cargo even there.

Be it farther observed, that the flux of air compressed into an half-inch stream, in its passage through the nosle, to enable it to overcome the resistance of the spiral spring H, no sooner passes the valve E, than it expands itself to the compass of the pipe; by which expansion, and extension (at the

* Thus calculated:

	Inches.	
60 feet	4	
× 12 inches	× 4	
Produces 720 inches as a multiplier.	16 × ,7854	the square of the diameter of the pipe.
	= 12,5664	or area of the circle.
	× 720	length of the pipe in inches.

Which, divided by 231)9047,8080(39,1679 gallons and decimal parts, the whole capacity of 60 feet of pipe.

2117

..388

1570

1848

221, &c.

Then, a single discharge of the forcing-pump being eight gallons, five such discharges amount to 40 gallons, which is more than equal to the contents of 6 feet of four-inch pipe.

And as on the larger scale of ventilators the pipes need not exceed the same diameter, the power of the air injected, when its egress is stopt, will increase sufficiently to force its way through webs, mats, and other obstructions, though impervious to the atmospheric fluid, unassisted by such mechanic aid.

same

ſame time) forwards, its power becomes ſo weakened, that ſmall egreſs only will be made, till the pipes are filled with a fluid more denſe than atmoſpheric air, which will then, as is juſtly noticed, iſſue where it finds the leaſt obſtruction, unleſs attracted to the ſpot moſt heated.

Many circumſtances may cauſe one part of the cargo to be leſs permeable than the reſt; ſhould it prove ſo, the means readily offer for airing and purifying even this.

Suppoſe the hatches to be caulked down, and the hold made impervious to water; in ſuch caſe, the lapſe of air, under the obſtructions met with in its paſſage, could by no means keep pace with the influx from the forcing-pump; conſequently, if the holes in the deck, deſigned for its exit, be kept cloſe-ſtopped till the pumper feels reſiſtance, all the intervals of the cargo, be they ever ſo minute or irregular, muſt be occupied by freſh air, which, when permitted to eſcape, will carry off impurities with it. And thus, by ſtopping and opening ſuch vents repeatedly, no part of the cargo could miſs of purification. and this perhaps may be the beſt mode of adminiſtering it.

Prevention is better than a cure.

In a veſſel equipped with the apparatus deſcribed, the inattention muſt be great, if the corn be ſuffered to ſuſtain any injury at all. By an early uſe of it, perſpiration and damps will preſently be dried away; heating of courſe will be prevented; and even the production of the pernicious grub alluded to: for, be the nidus of its eggs ever ſo productive, their embryos will not vivify, without moiſture to ſuſtain them. Wherefore, it ſhould ſeem that the corn-merchant in future will have little to fear, ſave the influx of ſea-water; and even this (if in ſmall quantities) will, by the frequent uſe of the ventilator, gradually dry away.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

ROYAL SOCIETY OF LONDON.

ON the 30th of November laſt, the anniverſary meeting of the Society was held. Sir Godfrey Copley's gold medal, which is diſpoſed of yearly on this day to the moſt deſerving member, was awarded to the Rev. Mr. Hellings.

The receipts of the Society for the year were declared to have been 1736 *l.* and the diſburſements 1563 *l.*

The meetings of December the 5th and 12th were occupied principally in reading the concluſion of Dr. Herſchel's paper on the power of penetrating into ſpace by means of teleſcopes. According to the Doctor's obſervations, it is impoſſible with the naked eye to ſee any ſtar ſmaller than thoſe of the 8th, 9th, or 10th magnitude. With his 40 feet reflector he has diſcovered ſtars which muſt be three hundred thouſand times more diſtant than the neareſt fixed ſtar. Where are the bounds of creation!

At the latter meeting, an abſtract of a regiſter of the weather at Lyndon in Rutlandſhire, kept by Mr. Barker, was read; as was alſo a paper on annuities, reſpecting the contingencies of three lives, by Mr. Morgan.

December 19th. A paper, by Mr. Jordan, on the irides ſurrounding the ſun, moon, &c. was partly gone through, when the Society adjourned for the Chriſtmas holidays.

January 9th, the concluſion of Mr. Jordan's paper was read; as was alſo a paper by Mr. Anthony Carliſle, on ſome peculiarities in the arterial ſyſtem of certain animals.

On the 16th and 23d, a paper on ſound.

COMET.

C. Lalande has announced that, on the 26th of December laſt, his fellow-labourer Mechain diſcovered a comet near the ſtar ς of Serpentarius, which could be diſtinguiſhed by the

the naked eye. It had a tail of a degree, and was proceeding in a ſouthern direction.

SUBSTITUTE FOR CINCHONA.

C. Zannetini, a phyſician who attended the French army in Italy, has made ſome experiments, by which it appears that the flowers and ſeeds of the common nettle (*Urtica dioides* LINN.) may be employed in fever inſtead of cinchona. This ſubſtitute was attended with a ſucceſs beyond all expectation, in tertian and quartan malignant fevers. The nettle often produces a ſpeedier effect than bark, for it heats in a great degree, and, when the doſe is pretty ſtrong, occaſions a lethargic ſleep. The doſe muſt never exceed a dram, and is given in wine two or three times in the courſe of 24 hours. Zannetini found this medicine of great ſervice to guard againſt that total exhauſtion which forms the principal character of malignant fevers; and he recommends a ſlight infuſion of it in wine as an excellent preſervative for thoſe who reſide in marſhy and inſalubrious diſtricts. In employing the nettle in fever, Zannetini gives the ſame caution as ought to be obſerved in regard to cinchona, that is, that it muſt not be employed where there is an inclination to inflammation, or where a continued fever, ariſing from obſtructions, exiſts. This diſcovery is not unworthy the attention of phyſicians, and deſerves at leaſt to be farther inveſtigated, as a great deal would be ſaved if cinchona could be entirely diſpenſed with.

CURE FOR DISEASED ELMS.

C. Boucher, ſecretary to the Society of Emulation at Abbeville, has lately publiſhed a memoir on the diſeaſes which attack elms, and the method of curing them, from which the following is an extract:—"Elms are frequently attacked by ulcers, which at length deſtroy a great number of theſe valuable trees. Duhamel ſuppoſed that this malady might be aſcribed to a plethora of the ſap; and C. Boucher, by numerous experiments, has eſtabliſhed this fact, and diſcovered a remedy. He obſerved, that local ulcers never attack the tree on the north ſide, but almoſt always on that expoſed to the ſouth. The elms chiefly ſubject to it are thoſe planted in marſhy ground, and in the neighbourhood of rivers. The ulcer is generally at a little diſtance from the earth, ſeldom

more than five or ſix feet above it. This diſeaſe, ariſing from a ſuperabundance of ſap, differs from another diſeaſe of the elm deſcribed in the *Journal d'Hiſtoire Naturelle** for the year 1789, in this circumſtance, that the liquor, when expoſed to the atmoſphere, ſoon acquires the conſiſtence of a gum, and has a very ſaccharine taſte.

To cure the trees attacked, C. Boucher pierced each of them with an auger, which he inſerted in the ulcer itſelf, and then fitted to the hole a tube which penetrated to the depth of 1·10 inches. Sound trees pierced in this manner yield no liquor; but thoſe which are diſeaſed give a more abundant quantity, in proportion to the ſerenity of the weather, and as the wound is more expoſed to the ſouth. This effect is ſuſpended by ſtormy weather and high wind. He obſerved that at the end of two or three days the efflux of the ſap ceaſed, and that the wound dried up and was healed.

This, therefore, is a ſimple and eaſy method of radically curing elms attacked with this diſeaſe. It is probable that the ſame proceſs applied to other vegetables, and particularly to ſome fruit-trees, would produce a like effect. Pliny, Columella, and Palladius, mention the ſame means as having been employed by the ancients, but it has not been before practiſed for many years.

C. Boucher has completed his obſervations on the elm, and proved that this tree is not an exotic, as ſome authors have advanced. The ſtudy of the ancients has proved to him that it exiſted in Europe in the remoteſt periods, and very evident remains of it have been found in old moſſes.

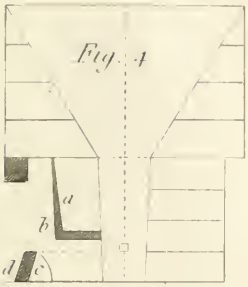
The analyſis which C. Boucher made of the ſap of the elm ſhewed that it contains a pretty large quantity of the acetite of pot-aſh, a little of the acetite of lime, a certain quantity of vegetable matter or ſaccharine mucus, and a pretty large quantity of the muriat of lime. There exiſt in it, alſo, ſlight traces of the ſulphat and muriat of pot-aſh. This is nearly the ſame analyſis as that before announced by C. Vauquelin.

NEW PNEUMATIC APPARATUS.

Mr. H. W. Pepys jun. has conſtructed a mercurial exhauſter on the principle of the Torricellian vacuum. The

* No. 5 and 6.

inventor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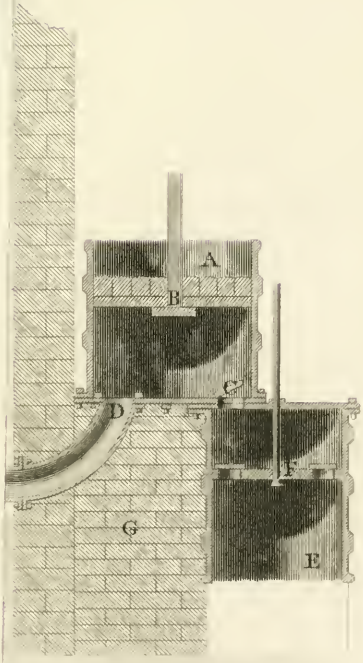
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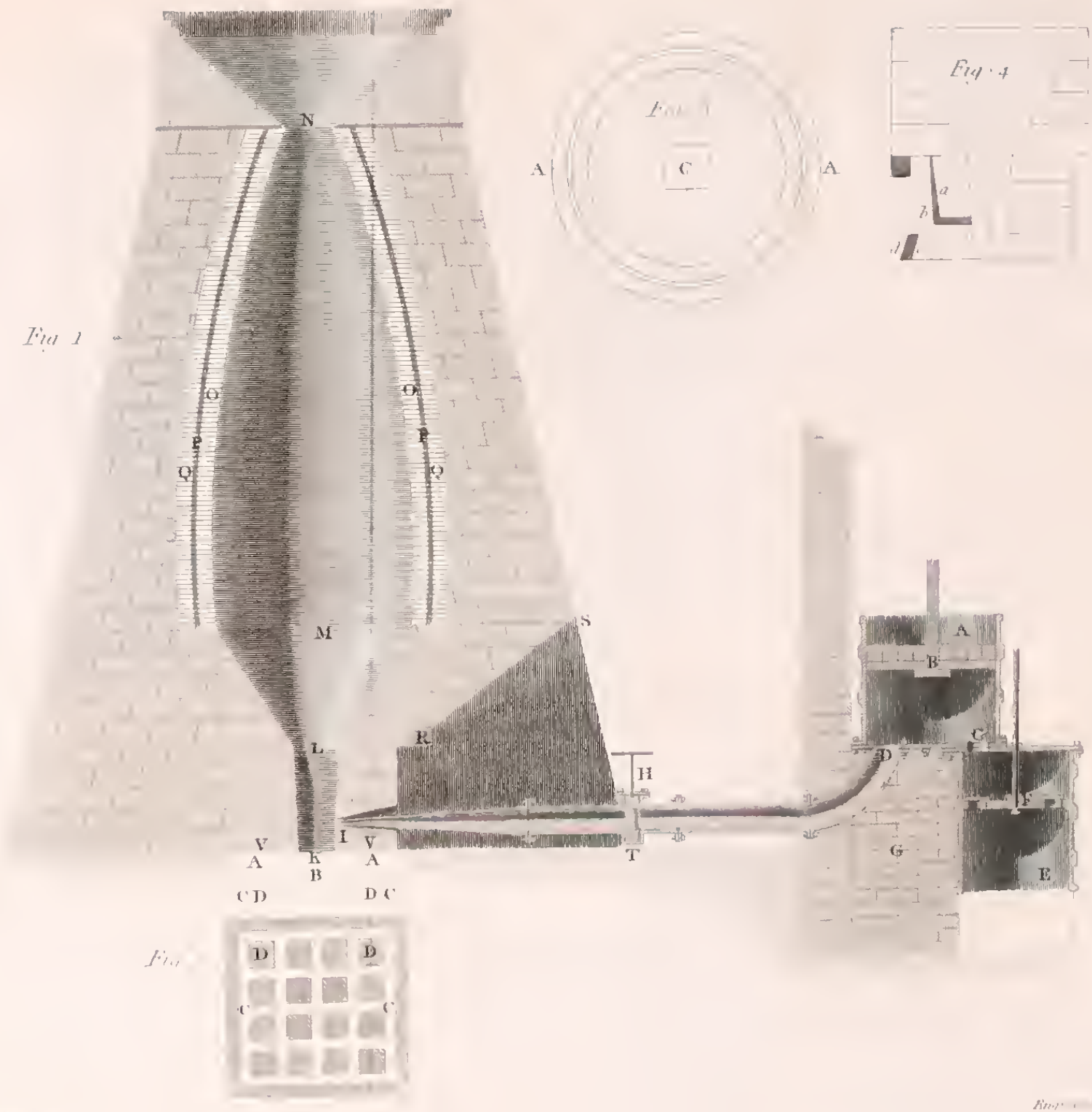
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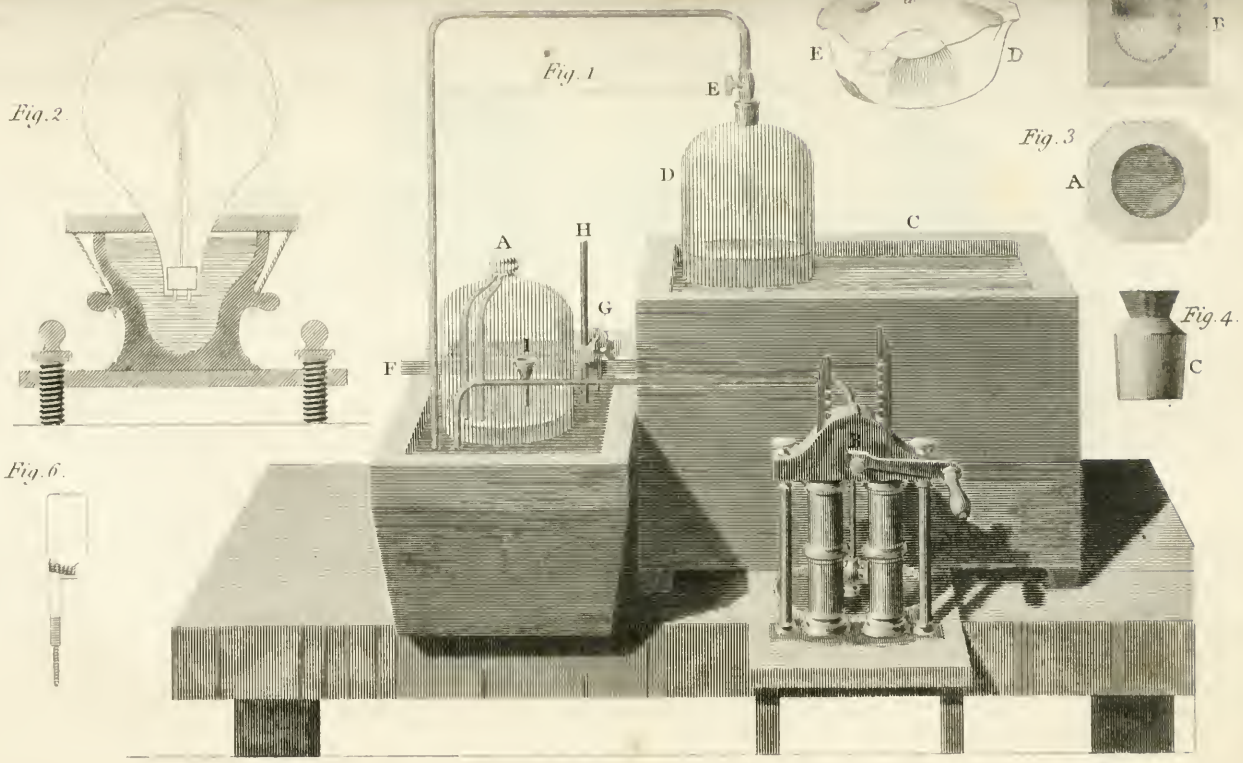

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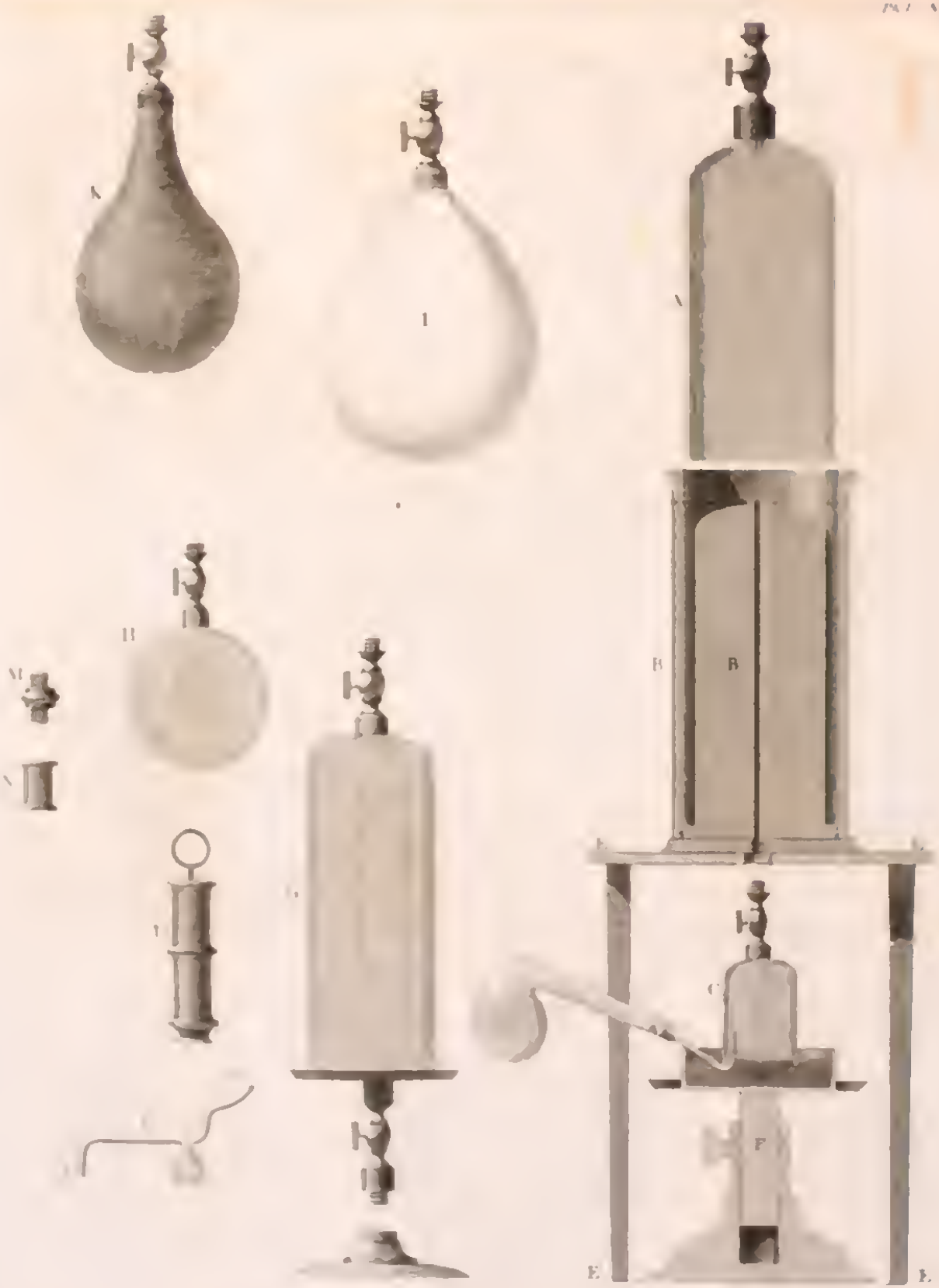
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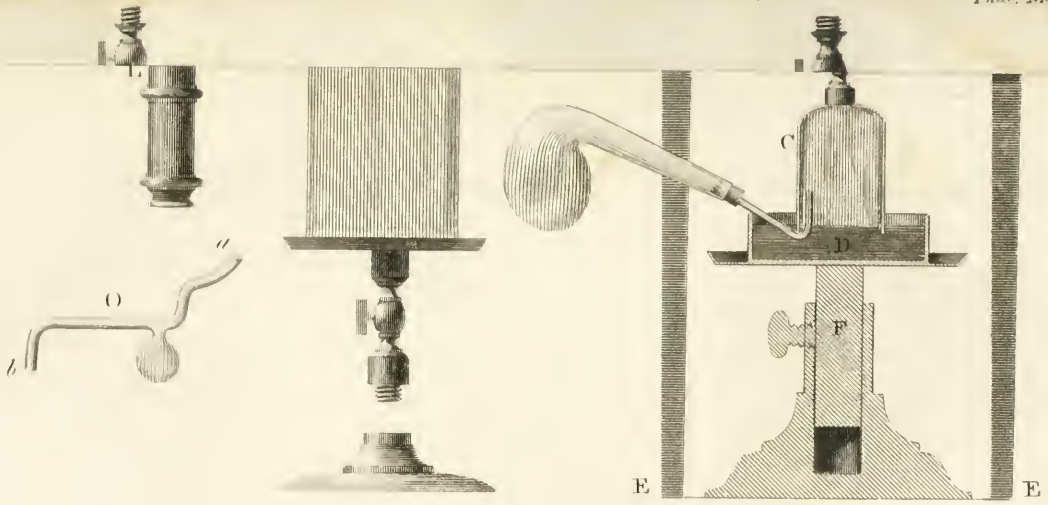

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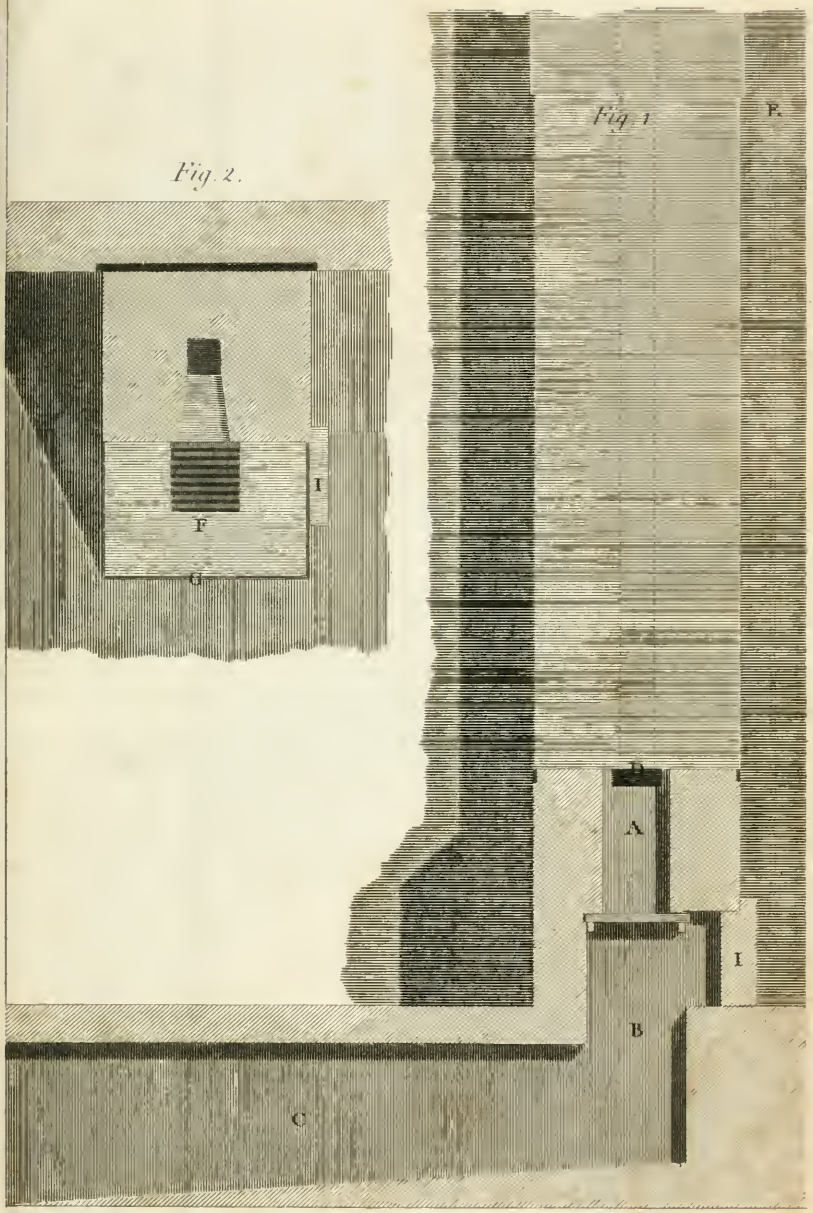

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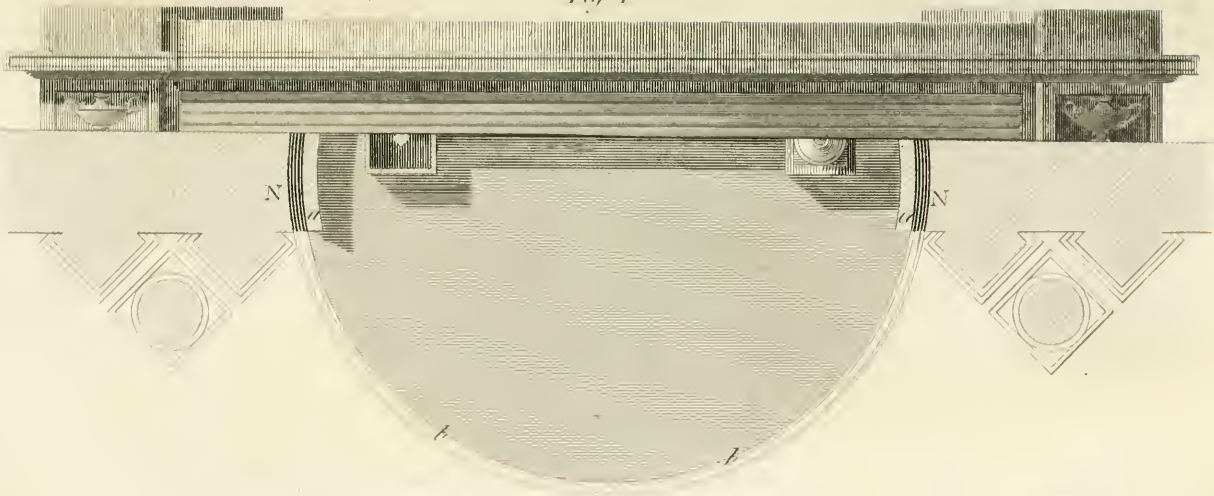

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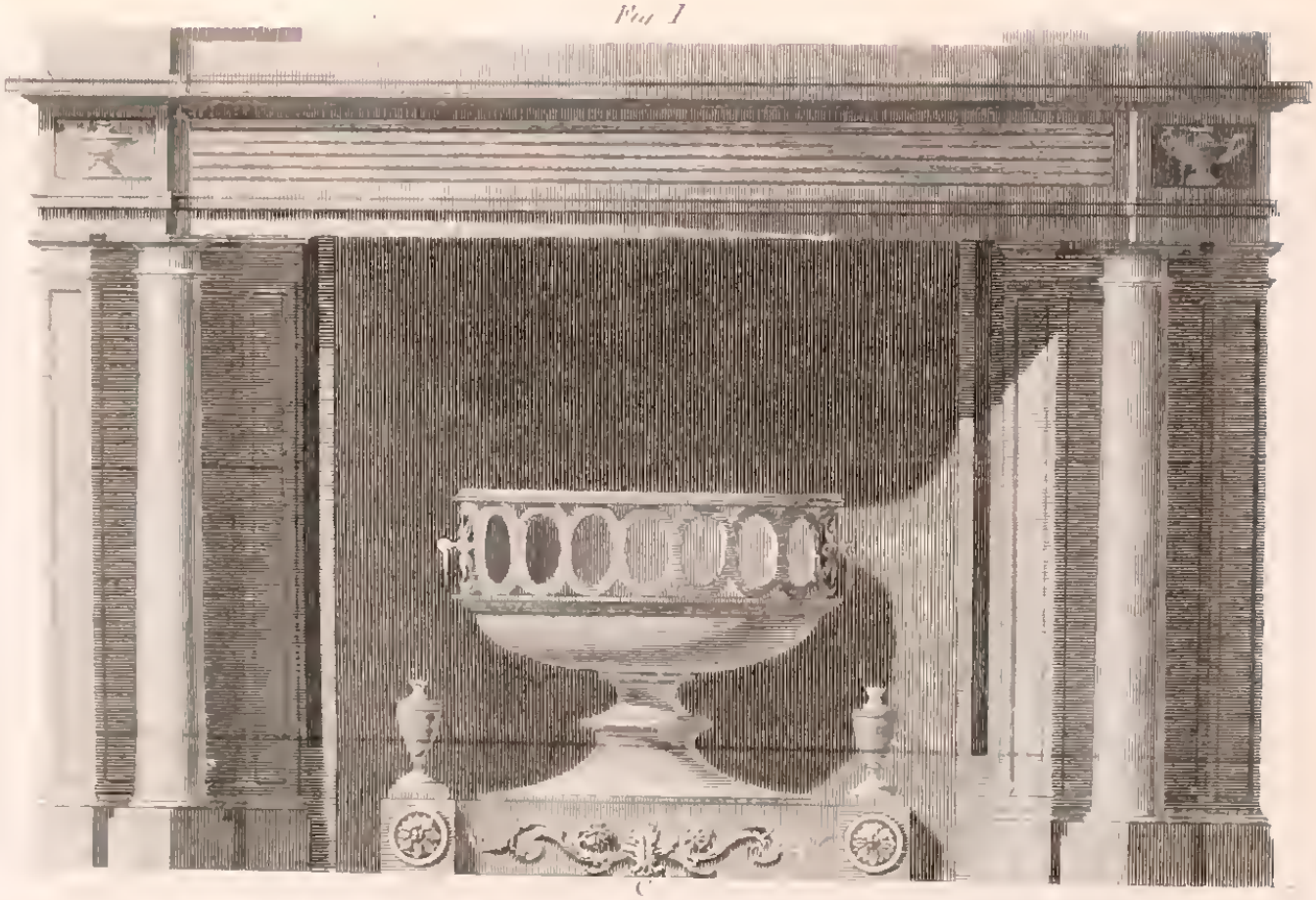

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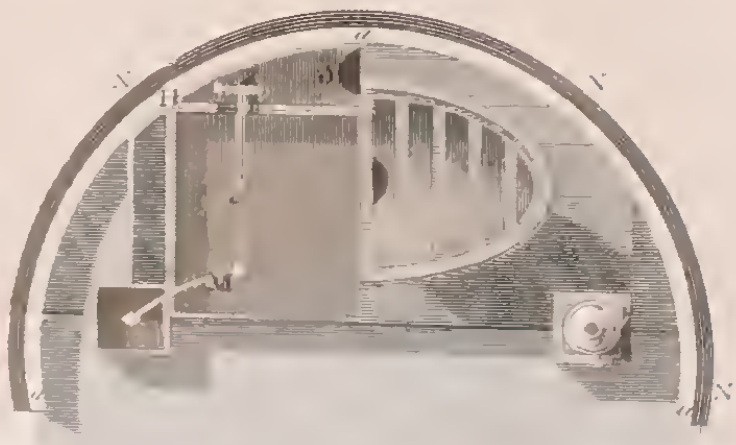

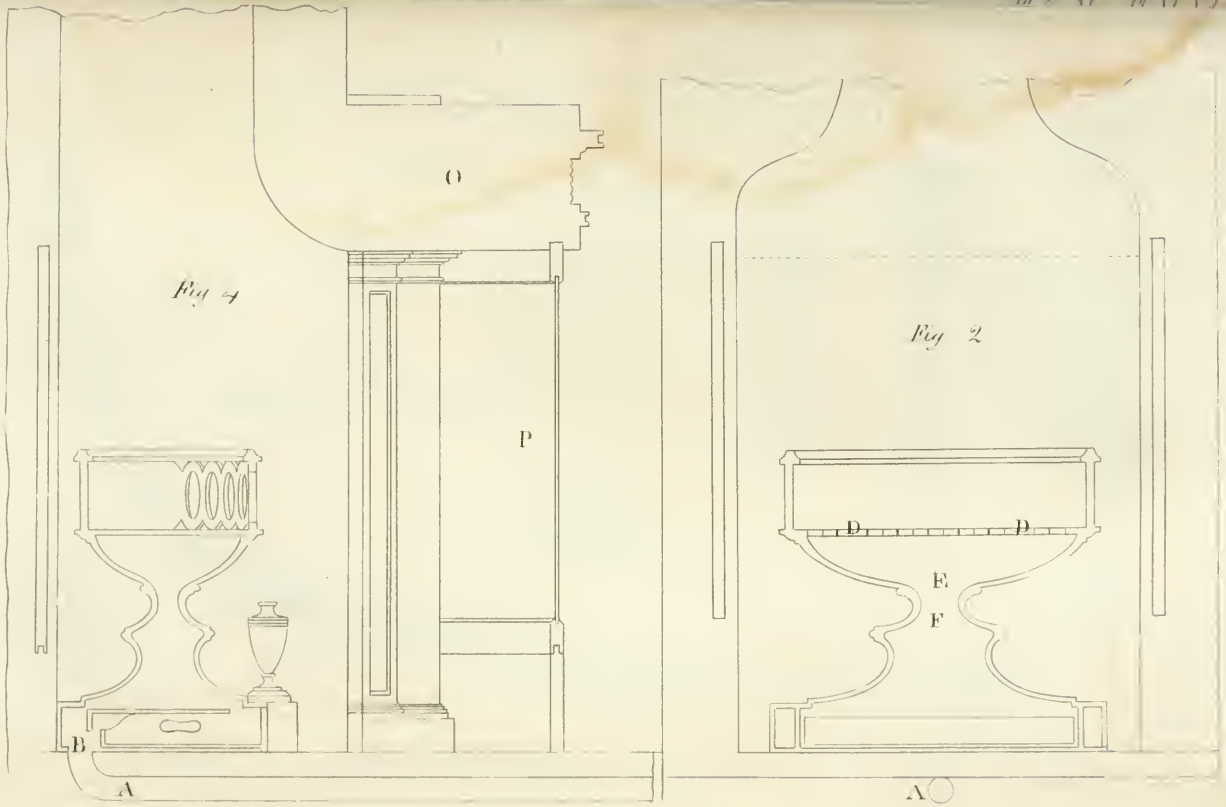
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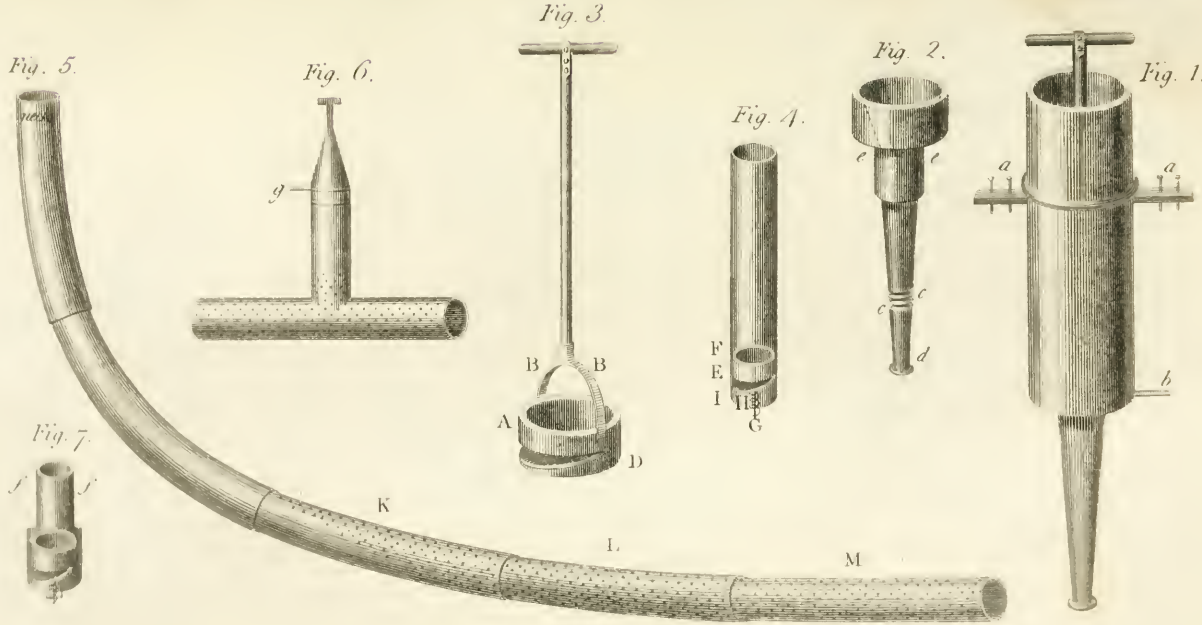
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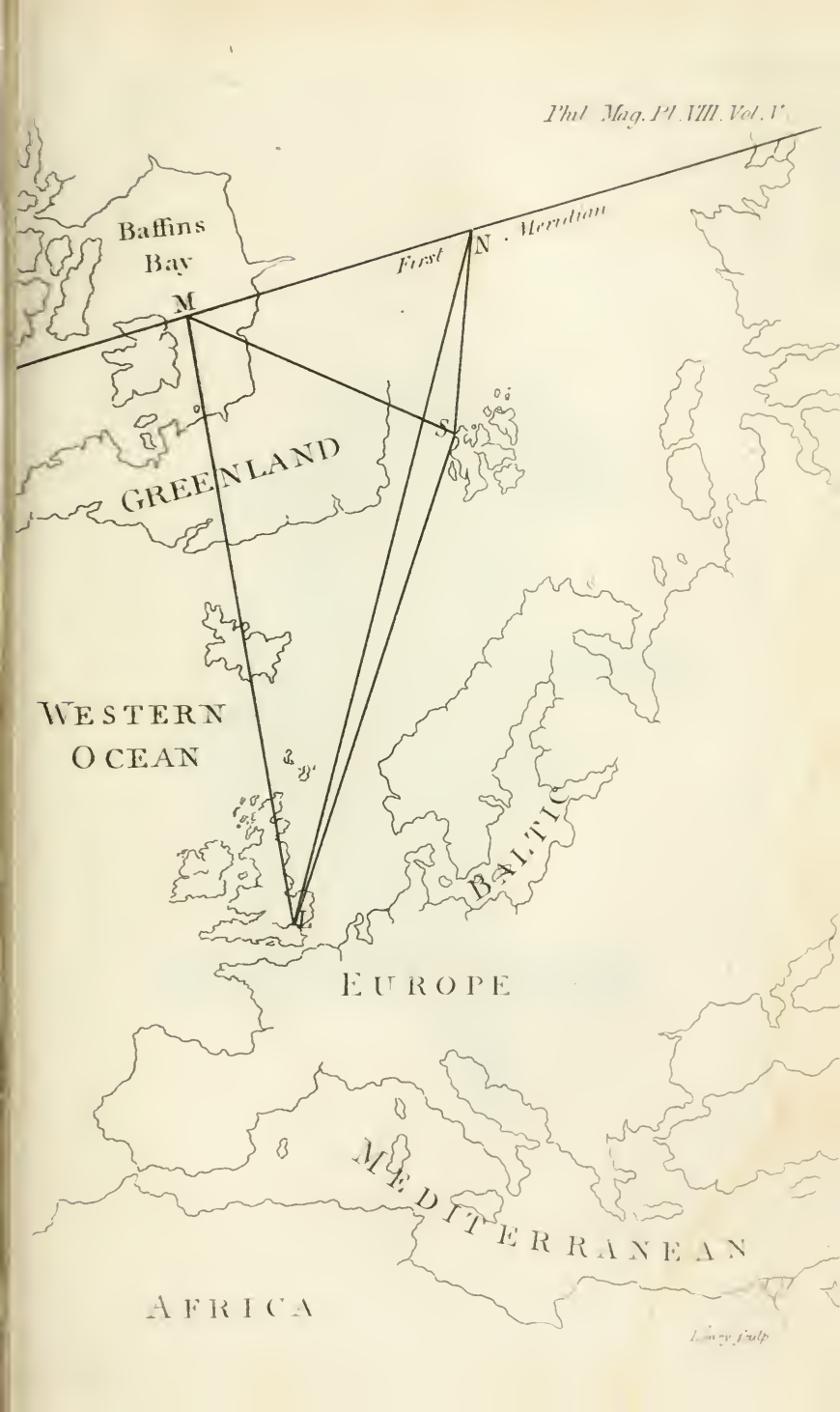
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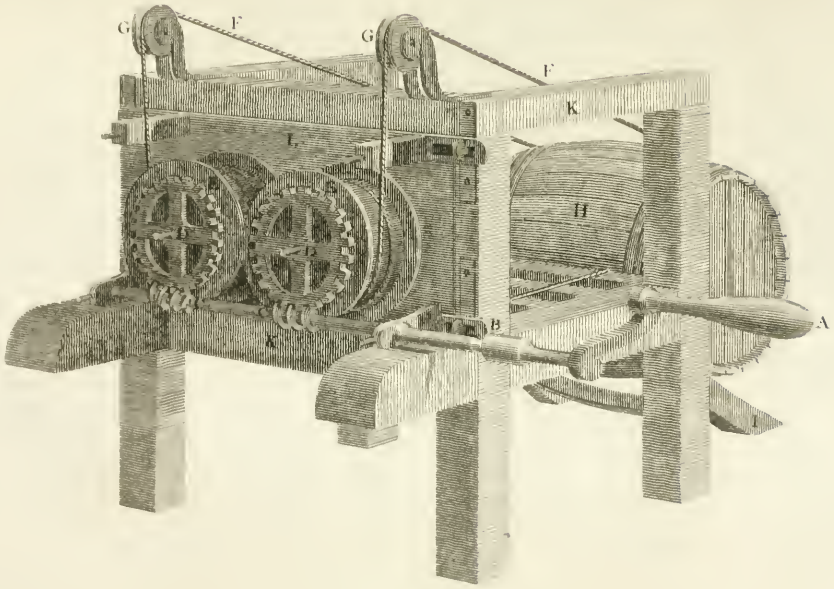
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inventor has promiſed to enable us to lay the plan of the apparatus, with ſome of the moſt intereſting of thoſe experiments which have yet been made with it, before our readers in a future number.

DEATHS.

On the 19th of December, C. Montucla, author of an excellent hiſtory of the mathematics. Two volumes only of the new edition of this work were publiſhed before the author's death, but fortunately all his corrections for the remainder were finiſhed.

At Paris, on the 1ſt of January, the celebrated naturaliſt Daubenton, the friend and fellow-labourer of Buffon. He was born at Montbard in Burgundy in the month of May 1716. He ſtudied medicine, and intended to follow that profeſſion in his own country; but Buffon, being appointed intendant of the king's garden in 1735, propoſed to Daubenton to reſide with him, to apply to natural hiſtory, and to aſſiſt him in the grand labours which he was then about to undertake. In 1740 the fate and taſte of Daubenton were determined for his whole life. More than half a century devoted to the formation of the cabinet of natural hiſtory, which in 1750 was only a plain drug-ſhop belonging to Geoffroy, which he arranged methodically and enriched with productions of every kind, has given him a diſtinguiſhed rank among naturaliſts. Being admitted into the Academy of Sciences in 1744, he never ceaſed to enrich the collection of its memoirs with various papers for nearly fifty years. The greater part of them contain new facts and ideas reſpecting the claſſification of ſhells, on the hippopotamus, the ſhrew-mouſe, bats, foſſil bones and teeth, the ſituation of the great occipital foramen in man and animals, rumination and the temperament of ſheep, a deſcription of ſeveral kinds of new animals, or animals little known. He was interred with great pomp on the 3d of January in the garden of plants. His funeral was attended by more than 500 perſons, and two orations were pronounced on the occaſion by C. Lacepede and C. Fourcroy.

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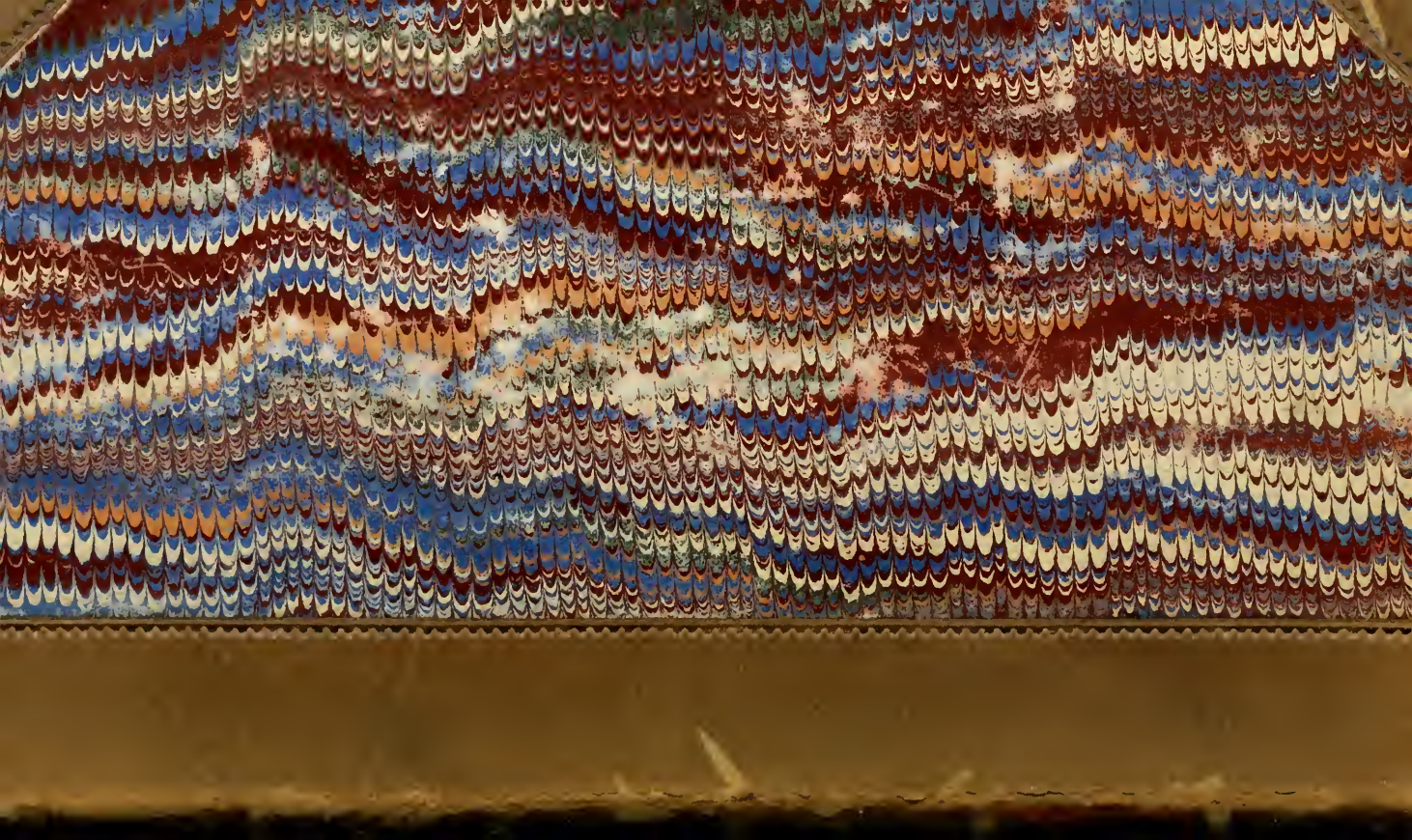
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